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COURSE OF

LECTURES ON THE BLOOD,

AND ON

THE CHANGES WHICH IT UNDERGOES DURING DISEASE,

Delivered at the College of France in 1837-8,

BY

M. MAGENDIE.

LECTURE XXIII.

Properties of the globules of the blood; their dimensions, structure, &c., in different animals. Infusoria of the globules. Globules of reptiles and birds. Functions of the globules. Their relations to each other in different animals. Substances which act on the globules. Experiments on animals. Cyanosis.

GENTLEMEN:—The close of the session is so near at hand that I shall be obliged to pass rather rapidly over several of the topics connected with the history of the blood, reserving for another time their further and complete study. Thus, we must content ourselves, for the present, with the few facts we have ascertained respecting the albumen, and proceed briefly to examine the chief properties of the globules.

The discovery of these bodies was coeval with that of the microscope, and, indeed, their history is hardly more perfect at the present day than it was during the time of Leuwenhoeck, to whose zeal we are indebted for the earliest observations made on the subject: up to the present hour we know nothing of their uses. Now, though this state of things is not what might have been expected from the almost daily improvements in our scientific and practical acquaintance with opties, yet we may reconcile ourselves to it, as it has left for us of the present day a vast and most interesting, but at the same time most difficult, material for study.

Such is the variety of their forms, dimensions, structure, &c., in different species of animals, that our first care must be, not to indulge too freely in generalisation, and attribute properties to those of one class of animals which have been discovered in another. Not attending to this point has proved the source of very serious errors. For this reason it is advisable to describe the globules separately, as they exist in the mammalia, in birds, fishes, &c. The principal points of view under which it behoves us to consider them are their structure, form, dimensions, and properties, and the effects resulting either from the influence of chemical agents on them, or from their contact with each other. I shall be the more particular in my examination of the relations they bear to each other, as certain physiologists of the present day, instead of recognising in the oscillations and displacements undergone by the globules, an application of the most simple laws of physics, have, in order to explain these movements, made so many little beings of the bodies in question, possessed of a will, going to the right and to the left, forwards and backwards, as the fancy may take them, and resisting in the capillary vessels, sometimes successfully too, the impulse communicated to them by the heart.

We shall first endeavour to obtain an accurate idea of what bodies are called globules; for, as I said to you formerly, there are various kinds found in the same blood. In the human subject, for example, there are some that exist constantly, others non-persistent. Among the former are to be ranged the red globules, of different forms and dimensions; among the second we find the large white globules, of which the uses are equally unknown, though they deserve investigation at the hands of observers, because they form some of the normal constituents of the blood. Along with these are found other globules, infinitely smaller than the red and white ones, which it has been fancied belong to the lymph or chyle. So far, however, this opinion respecting their nature is merely conjectural.

The discovery of the two last orders of globules has been made within the few last years; and there is a third description, of still more recent discovery, to be mentioned. These are, adventitious: it would appear that in the course of certain diseases, glo-

bules of particular structure and appearance are developed. Indeed, this is a demonstrated fact, and you may consult the works of Müller and Burdach respecting it with much advantage.

Here is the manner in which the globules are generally obtained for study. Some blood is received into a vessel, and beat up with rods; the fibrine is thus separated from the serum, and the globules gravitate to the bottom. A few of them are then spread on the object-glass of a microscope, and by the help of this instrument their circular form is recognised. There are some precautions necessary to be taken in order to make these observations, simple as they may appear. Numerous fluids, water for example, dissolve or alter the properties of the globules; care must, therefore, be had to receive them in the serum, their natural vehicle. Another process for procuring them consists in pricking one's finger with a needle, and spreading the drop of blood thus obtained on a plate of glass; but in this way the globules are so numerous that it is impossible to distinguish them accurately, unless the precaution be taken to suspend them in a small quantity of saccharine or saline water, which, as you are aware, do not dissolve these minute corpuscula.

I told you that their form is circular; but the point which strikes the observer most strongly after the first glance is, that the centre of these bodies has not the same appearance as their circumference. In the former position is seen a black or white point, according to its degree of nearness to the focus of the instrument, and the quantity of light that falls on it; so that sometimes one would fancy them perforated in the centre, while in other cases a sort of nucleus, distinct from the mass of the globule, is very perceptible. Both these opinions, founded as they are on direct observation, have been maintained by physiologists; but here, as in many other cases, the apparent has, I think, been taken for the real. However, it is extremely important to learn to distinguish the centre of the globules; for, according to some physiologists, it contains a solid nucleus in mammiferous animals; whereas, in the opinion of others, and I am among the number, it is more depressed and thinner in this point than elsewhere. I am induced, for the following reason, to deny the existence of a central nucleus in the human subject. If you take some blood, either of a fish or a reptile, and place the globules it contains in the focus of your microscope, you will distinctly perceive a peculiar swelling in their centre; next dissolve the globules, and examine the water used for the purpose; you will find no globules in it, but instead, the minute bodies that formed their centre, for these are insoluble in water. Now, in the case of the human being, water dissolves the globules completely; no vestige of them is discoverable after the water has acted fully. This fact appears to me conclusive of the correctness of the theory I have adopted, and to that theory I shall consequently adhere until some very strong reasons indeed against it have been made evident. I am aware that M. Letellier, a gentleman well skilled in microscopical observation, affirms that water does not dissolve the globules of the mammalia more perfectly than those of reptiles, and that when these bodies are shaken in the liquid named, a multitude of minute corpuscula, which are, in his opinion, nothing more than the central nuclei, are precipitated to the bottom of the vessel. But on repeating the experiment I fancied I ascertained that the deposit was formed of incompletely dissolved globules. The corpuscula in question were of the same size as the globules themselves, and seemed to differ from them simply in being totally decolourised.

The exact dimensions of the globules are learned by means of a very simple instrument called the micrometer. It is composed of a small plate of glass on which a scale of hundredths, two-hundredths, and even five-hundredths of a millimetre is engraved. The divisions of the scale are placed in the focus of the microscope, the magnifying power of the instrument being known; the globules are then placed there also, and those minute bodies are compared with the divisions of the scale. A very simple calculation then tells how many hundredths of a millimetre they embrace. By these means we have learned that the ordinary size of the red globules of human blood varies from the one-hundred-and-tenth to the one-hundred-and-twentieth part of a millimetre. From this brief statement you will readily understand that we might, after ascertaining their thickness and superficial extent, calculate exactly how many globules a single cube millimetre of blood must contain, and so, were we acquainted with the exact volume of blood in the frame, the infinite number existing in the entire circulating system.

The term globule is inappropriately applied to these bodies, for their form is not spherical but lenticular. What proves this, and the fact can easily be ascertained, is, that when they roll under the microscope they turn their edge to the eye of the observer. This edge generally measures in point of thickness the fifth or sixth part of their superficial extent; seen in this manner the globule appears thicker than at its middle, which part has the appearance of being slightly depressed, and, as it were, excavated; this is, however, the case only with the globules of mammiferous animals, for those of others, such as reptiles and fishes, present a real swelling in their centre.

Continuing our microscopical examination of the movements of the globules, we observe them bending, twisting, rolling over each other, until they gradually recover their original appearance. But their intimate structure is not easily determined; according to some they are formed of an exterior investment and a central nucleus, like those of reptiles and birds; but observations, repeated over and over again, militate strongly against the correctness of this notion. In truth, even with a magnifying power of eighteen hundred times, I have never been able to satisfy myself of the presence either of an investment or of a nucleus. It is possible (but I would not affirm that such is the case) that these bodies are provided with an investment which tears. Observers are, indeed, generally of opinion that they are surrounded with a very delicate pellicle; and this idea receives some support from the fact that in the globules of dead subjects there is a sort of puckering visible, such as is presented by membranes of extreme thinness when they begin to dry, for instance, the outer skin of onions.

M. Donné, whose zealous and successful cultivation of microscopical researches is well known, conceived that this phenomenon might be looked on as a certain sign of the death of the individual in whom it was observed,—an idea which might have had a certain degree of importance in forensic medicine. But I have ascertained that the same phenomenon occurred in the globules of a healthy individual, full of life, when they had stood for a certain time in a vessel. Nevertheless, this fact seems to indicate that the globules are enveloped in a sort of membrane, and that this membrane is soluble in water, the acids, the alkalies, and a variety of other fluids; consequently they must possess some peculiar properties which enable them to retain their form; otherwise, as the blood contains water and various salts, the globules must of necessity be dissolved. I must endeavour to make out what these properties are; and, with this view, I shall make an artificial serum, and observe what action it exercises on the globules. It has been stated that they owe their persistence in the lenticular form to the albumen; I will separate the albumen from the serum, which is the best way to ascertain if the blood would be unable without that principle to preserve its globules uninjured.

If you wish to observe the globules in the possession of their normal properties, you must take care that they are recent; otherwise you will see them when they have undergone serious changes, partial destruction of their edges, while their surface is mottled with minute spots; they now resemble a combination of small masses constituting an amorphous and irregular whole. Here is another fact which I have recently observed. For the first two or three days no very evident change occurs in the globules; they then assume the maculated aspect just alluded to; when in this state I have given them the name of *corpusculaires*. A third period ensues, during the continuance of which they become the seat of movements of totality, resembling those of infusoria; it is this which has probably made various authors fancy that the globules were, in truth, so many animalcules. These movements, which are exceedingly evident, resemble what are called in physiology *vibratile motions*, and are observable in various organs, especially in the mucous membranes of birds, and of the human subject. In the latter the borders only of the membrane are subject to these vibrations, whereas the centre even of the globule manifests the phenomenon distinctly.

By means of the microscope it has been ascertained that the vibratile motions of the extremity of the lamelles of oysters is due to the presence of minute animalcules; and, by a very surprising analogy, I have distinctly perceived small vibrions on the globules, moving on their surface, penetrating into their substance, and issuing from it by the edges. Finally, the globule diminishes in size and gradually disappears; it appears to be devoured by these infusoria, which has not been mentioned, I believe, by any other person. Some globules present a multitude of these animalcules; others a very few. Thus, the blood furnished by a nasal polypus which I excised lately, contained an innumerable quantity of them.

Hence it appears that, in order to commit no errors in our observations on the globules, we must know how long the blood has been removed from its vessels. This is a fact hitherto unknown; and I cannot believe that I am in error in announcing it now, for, as it was perfectly new to me as well as to others, I repeated my experiments frequently, and, in every instance, with the same results. As to the variations in volume occurring in different animals, you may profitably consult the analytical table of MM. Prevost and Dumas; I have recently found with M. Poiseuille that those of a torpid bat measured the one-hundred-and-thirty-fifth part of a millimetre.

If we examine the globules of another order of animals, reptiles for example, we shall find them possessed of a different structure and conformation. One of their diameters is evidently more elongated than the other; they present a very distinct spot in their centre, and when they turn on their axis a prominence is visible on their edge. When shaken in water they dissolve, except the central part, which is wh... when it has been well washed, and retains the elliptic form of the original globule. Besides these globules, there are other spherical opaque corpuscula to be seen without any nucleus.

It is possible that these are either nuclei separated from the elliptic globules, or these same globules in course of formation: but, after all, I do not see the necessity of trying to make out their nature by mere hypotheses; the truth is, that I know nothing about them.

The researches which have been made on the nature of the investment and nucleus of the elliptic globules, have led to a belief that the former presents much resemblance to the colouring matter of the blood, and have also shown that the whitish matter of the nuclei, when treated with acetic acid, forms into small, tremulous, and gelatiniform masses.

In birds the globules are elliptic, but they have no nucleus, and hereupon have arisen a host of hypotheses. It has been said, for instance, that they resembled the pollen of flowers; that, like it, they consisted of myriads of infinitely small corpuscula united in a heap; this, and the other suppositions to which I allude, are, no doubt, pretty and ingenious, but they are deficient in one essential point, the material proofs of their reality. The difference in these globules has reference not only to their structure but also to their size: the globules of reptiles have a mean size of from the forty-fifth to the seventy-fifth part of a millimetre; they are, consequently, much larger than those of mammiferous animals and birds.

The function of the globules in the act of the circulation is unknown, and, I fear, in spite of the laborious researches of which the blood is the subject, will long continue so. I have endeavoured to make elliptic globules pass through the blood-vessels of several of the mammalia; they all died, because the injected blood had coagulated in the syringe, and not on account of the want of proportion between the globules and the tubes to be traversed. I have also injected globules separated from the fibrine; the subjects of this experiment suffered no inconvenience from its performance. Here is an animal into whose veins I have injected the globules of three other dogs, stronger than himself, and yet, to all appearance, he is in a state of perfect health.

Allow me, Gentlemen, to interrupt for a minute or two my remarks on the globules, while I say a few words on a case to which I have already alluded. You, without doubt, remember the woman affected with chorea, whose history I related to you. All imaginable kinds of treatment were in vain tried for her relief; and, in the course of time, a complete luxation of the femur from the tibia formed, so complete that all contact between the articular surfaces had ceased to exist. The condyles of the thigh-bone appeared, anteriorly and internally, the head of the tibia posteriorly and externally. This singular luxation caused the rupture of several small blood-vessels, and laceration of the neighbouring tissues. The internal surface of the articulation was laid bare, and the line of union of the cartilage with the synovial membrane plainly visible. The cartilage was, as it were, dissolved and converted into a pultaceous and creamy matter; to this condition succeeded the formation of a sort of granulations, and these soon assumed a dermoid appearance, such as is observed to occur in the mucous membranes in cases of artificial anus and prolapsus uteri.

This case furnished me with a new opportunity of ascertaining that the synovial membrane does not invest the articular cartilage, as Bichat supposed it did. This opinion was one of the numerous seductive hypotheses with which that brilliant writer loaded the science of physiology. Others have supposed that the synovial membrane passed not over but under the cartilage,—a notion which is equally inadmissible; I should certainly, in the case of which I speak, have perceived the membrane in that situation had it really existed there. Another very remarkable fact, in a physiological point of view, connected with this case is, that the cavity of the joint was in contact with the air for two entire months, and yet, during that period, no fever or other disordered condition supervened. Yet we hear it daily announced with the utmost gravity from professorial chairs, that the introduction of atmospheric air into the articulations is a circumstance fraught with most injurious consequences. The granulations of which I spoke were developed in every part of the articulation, even on the fat situated behind the patella. The circulation was kept up in all these points.

I at first hoped that an ankylosis would have been the natural result of this really extraordinary dislocation; and in order to facilitate its formation every species of fracture apparatus was tried in turn, but the sudden and involuntary movements to which the limbs of the patient were exposed by her nervous disease, constantly put the splints out of place, and each day the luxation appeared still more marked than on the previous one. Finding the case thus obstinate, I considered it right to have recourse to amputation, though rather, indeed, as a means of relief than of cure. I performed the operation this morning without the patient's having felt the least pain during its performance,—almost without her having perceived what was going forward. Here is the limb; I have caused it to be brought here that we might examine together the lesions of which it is the seat.

I proceed cautiously, for I look on the case as a very curious one; in the first place you see that the synovial membrane is puffy, *inflamed*, as people say; you observe that on reaching the cartilage it becomes

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continuous with the granulations. The whole interior of the joint is imbibed with blood; the internal lateral ligament is totally destroyed; the external almost healthy; the crucial ligaments have disappeared, as also that inserted into the tuberosity of the tibia. It would be a difficult matter to find another case resembling this in the advanced nature of the lesions I show you, and the total absence of anything like constitutional reaction; and besides I know not how to find a satisfactory explanation of the extensive disease of this limb unless by having recourse to the *phlogistic theories*, wherewith all difficulties, as you are aware, both great and small, may at once be settled. But I have too favourable an opinion of your understandings, Gentlemen, to set about trying to amuse you with superannuated and ridiculous ideas.

In performing the amputation I employed a process of which I may as well give you a brief description. It is the usual habit to place the arm under the thigh, and divide the tissues by turning the hand round the surface; but this method is inconvenient, because the blood flowing from the vessels falls on the hand or coat-sleeve of the operator. In the present instance I commenced the operation below, and, having brought my first incision to the bone, I then divided the deep layer of muscles in the same manner; I took no more time to perform the operation in this than in the ordinary way.

To return to the globules; when these little bodies are left to themselves for twenty-four or thirty-six hours, they undergo, as I mentioned, a notable alteration in their appearance, they become puckered; besides this, a multitude of monads, or vibrions, appear at the same time in the serum; the gradual destruction of the globules effected by these new-comers, I have already adverted to. I felt anxious to learn whether these monads would attack fresh globules, and to decide the point put some globules extracted from newly-drawn blood into serum containing a large proportion of infusoria. I observed that they pounced on them with a sort of fury, and destroyed them totally in a very short time. It is very useful as a means of seeing the form of the globules, to bring them within the reach of these animalcules, inasmuch as they turn and twist them in every direction, and so allow the eye of the observer to ascertain their configuration with much accuracy.

Some globules of rabbit's blood mixed with the serum containing the infusoria, did not appear to excite in them the same degree of activity as those of human blood. I next tried them with elliptic globules, extracted from the blood of a bird; they turned them about, but almost immediately abandoned them. They seemed to regard with even stronger repugnance the globules of frog's blood, which are still larger. I saw them rush in troops to them, but almost immediately quit them, as it were, in search of better fortune elsewhere.

With respect to the relations of the globules to each other, here is what I observed. Those that are circular, when placed in serum, adhere to each other, and form flexible piles of variable length. It is not long since it was believed that these globules, thus heaped and piled one on the other, formed the basis of, or rather actually constituted muscular fibre; but besides that muscular fibre has not the appearance of globules piled on each other, it is matter of demonstration that it contains none of them. The modes of agglomeration of these little bodies are so numerous and various that it is useless to attempt their description.

The elliptic globules of birds do not form piles or chaplets; they become mutually attached, but this attachment takes place by all the points of their surface, and especially by the extremities of their greater diameter; they thus constitute masses of a particular aspect. They are then seen to adhere to each other by a single point, instead of being super-imposed by their whole surface, as in other instances. On mingling circular and elliptic globules together, I found that those of the same form adhered to each other; it would appear from this that they are under the influence of the electrical phenomena of attraction and repulsion.

As for their structure, it is certain that the globules of reptiles contain a central nucleus, surrounded with a lighter-coloured areola. In birds, on the contrary, the middle part of the globule is occupied by a nebulous matter having the appearance of a nucleus.

It is these bodies, as you are aware, that give the blood its colour; the nature of the colouring matter of the blood is not perfectly understood; chemistry has not yet revealed its composition. It has, however, been demonstrated that the globules undergo modification during the act of respiration, and also by the action of different reagents, which either destroy or change their colour.

In the experiments made in the course of these lectures we have established clearly that some substances act energetically on these globules, whereas others have no effect on them. Among the former all the acids are to be ranged. As you see by the contents of this vessel, hydrosulphuric acid not only destroys their colour, but their whole substance. On the other hand, the bicarbonate of soda tinges them of a scarlet colour. Tannic acid changes their colour to a pale pink. Chemistry throws no light on all these phenomena, which, nevertheless, belong to its province. Let us hope, however, that it will one day be enabled to solve these questions, all of which, especially the intimate

composition of the globules, possess much scientific interest. On this latter point we have, as yet, nothing but hypotheses: thus, Everard Home, Prevost, and Dumas, consider them formed of a nucleus, the basis of which is composed of fibrine, and that the hæmatosine invests them in the living blood like a very thin bladder; but how is it possible to admit this to be their true structure, when it is clearly ascertained that the globules of man and other mammiferous animals contain no nucleus. Again, M. Donné thinks that they are composed of an extremely delicate net-work, containing albumen and hæmatosine within its meshes; but almost simple experiment shows the fallacy of this notion: compare, in truth, the action of water on the globules of mammalia and birds, and on fibrine; the former are not dissolved by it, the latter is insoluble therein. It has also been affirmed that they are formed by the albumen of the serum; but M. Lecanu has demonstrated the existence of very considerable differences between that substance and the globules. Other observers, M. Denis for instance, imagine that they are solely formed of hæmatosine. Now, in order to extract that substance from the globules these bodies must be treated repeatedly by sulphuric acid, alcohol, and ether; and it is an undoubted fact, that when they have been acted on by these reagents, their physical and chemical properties are totally altered. They then leave as residue a greyish powder, of which I show you a specimen on this saucer. When examined under the microscope this powder presents no trace of globules; it is brilliant, with a metallic aspect, and insoluble in water, and in dilute sulphuric and acetic acids,—fluids which dissolve the colouring matter of the blood perfectly. This hæmatosine retains the metallic part of the globules in the state of peroxide; and so large is the proportion of this oxide of iron, that it has been proposed to make medals of it; to its presence the globules owe their very considerable specific gravity. But there is no similarity between this hæmatosine and the colouring matter.

The question of the use of the globules in the circulation is still unresolved. I injected an enormous quantity of globules (two pounds and a half), taken from three dogs, into the veins of another animal of the same species. The dog died in a few days in a state of extreme weakness; his gait was staggering, like that of defibrinised animals. The autopsy, however, did not disclose the phenomena I should have expected to find in an animal who had died in the manner described. The lungs were, it is true, altered in appearance, and covered with petechiæ, as in defibrinised animals; but the blood had coagulated slightly in the vessels; the mucous membrane of the intestines was healthy. The results of this experiment consequently throw no light on the uses of the globules. Nor have I been more successful with some other trials of the same kind; thus, I injected 15 centilitres of turkey's blood into the veins of a dog at three o'clock yesterday afternoon; the animal died this morning. You see that its lung, which I show you, presents peculiar arborisations, and that, though it continues crepitating, it is engorged; the blood is liquid. But the most remarkable circumstance connected with this experiment is, that on examining the blood under the microscope, I found that no traces of the elliptic globules which I had injected in such large proportion were discoverable. It would appear that they underwent some modification in their passage through the capillary vessels of the animal; and this, if I may be allowed to hazard a supposition, would lead one to believe that the configuration of the minute vessels of mammalia and of birds differs.

I have twice repeated another experiment, consisting in the injection of the blood of twelve or fifteen frogs into the veins of a young dog. The globules of reptiles, as you are aware, are much larger than those of other mammiferous animals, and, besides this, they have a central nucleus, formed of a distinct element, different in nature from the colouring matter. In spite of all these differences of relation, of structure, &c., the injected globules disappeared totally. The animal that underwent the operation still lives, and to all appearance is in perfect health. I drew a small quantity of blood from him, and not a single globule of the elliptic form could I find, though I even brought them into contact with the infusoria of some serum, by whom they were turned and twisted in every direction. I am utterly unacquainted with the mechanism by which this disappearance of the globules in question is effected; but you will remark, Gentlemen, that this is not the first time a fact of this kind has presented itself to our notice. You must remember that the albumen of eggs lost its distinctive properties after injection into the veins of a dog and assumed those of the albumen of serum. However, one thing is certain, namely, that the abnormal globules traversed the capillaries of the lungs; were it not so, we should have seen the phenomena of obstruction, or, as they are called, of inflammation, developed.

But, as you may urge, is it not possible that these globules were arrested in their progress through other less important organs?—No, for were this the case, we should infallibly have observed morbid phenomena of the same kind as those produced by the globules of starch and dextrine, of a tenth or twentieth of a millimetre in diameter. As for the mixture of bird's blood with that of a mammiferous animal, we recognised in it some acicular crystals, which disappeared in the course of four and twenty hours, whether from the

disengagement of ammonia, or otherwise, I know not.

One of the most interesting questions regarding the history of the globules is to calculate approximatively the proportion which they bear in point of quantity to the other elements of the blood. In the present state of the science it is almost impossible to answer this question. If we defibrinise some blood there remain the serum and globules; the latter will in part fall to the bottom, and in part remain mixed with the serum, if the whole be allowed to stand. If we heat this, the albumen will become solid, but it will imprison the globules in its substance; and the same effect will take place if we evaporate the water of the serum in a sand-bath or in a vacuum. I am of opinion that the most likely way to succeed would be to dilute the serum with sugar and water,—a mixture which you well know does not dissolve the globules. Nevertheless, even in this way, there would still remain a little sugar and water, albumen, and some of the salts of the serum mixed with the serosity; but we should undoubtedly, by this plan, come nearer the truth than in any other way.

It appears to be an indubitable fact, Gentlemen, that the proportion of globules and serum undergoes increase or diminution under certain circumstances; thus, after a certain number of bleedings, the blood is rich in serosity, but poorly supplied with globules. In the class of affections called anemic, the colouring matter of the blood is to a certain extent lost.

If the globular material of the blood be kept in a vessel for some time, the globules change their colour, disengage ammonia, and end by disappearing altogether. Here is some blood taken from a person affected with hypochondriasis, which I have now had by me for several days. It is in a state of putrefaction, and no trace of globules discoverable in it. In this instance, at least, their disappearance is not the result of the disengagement of ammonia; for you see that on placing a piece of litmus paper in the fluid, it is immediately turned red,—an unanswerable proof that the fluid is acid, and not alkaline. We are, therefore, obliged to ascribe it in part to the infusoria of which I have spoken to you. These animalcules first appear like little black points, and gradually grow bigger: whether they use the globules as food, in the same manner as animals having a mouth, I cannot say, but certain it is that they destroy them.

On adding some common water to this blood I found that the vibrions appeared for a time to be benumbed, but they soon resumed their rapid and varied evolutions; a single drop of acetic acid put an end to them for good and all. That these are not the only animalcules found in the living economy, however, is certain. Thus, if you examine the black fringe which hangs from the edge of the choroid down to the pupil, in the eye of the horse, you will perceive a matter composed of minute granules, which granules are nothing more than masses of infusoria adhering to the membrane and communicating vibratile movements to it,—such, at least, they appeared to me. I think that this movement is not oscillatory, as described by M. Brown. The chyle, also, contains a large quantity of vibrions.

M. Donné imagines that the globules are so many utricles, or bags, containing a liquid. In order to ascertain whether this hypothesis was correct, I performed the following experiment:—I put some globules in oil; they dissolved in that medium, but no fluid made its appearance; the parts of the globules mingled with the oil, and, as you may see here, some few little masses of globular detritus are still imperfectly decomposed.

Independently of the red globules, there are others of another kind in the blood, as I mentioned to you, differing from those in point of size, conformation, and colour; they are known as the white globules. They are first recognised by their greater size, and, in addition, they may be known by having neither central spot nor prominence, but a small part of lighter colour than the rest, which gives them a peculiar appearance; they are flat and lenticular, like the red globules, and often stick to the piece of glass on which they are laid for examination; this last character will serve to distinguish them, for those of the coloured species float about, and oscillate hither and thither continually. In order to isolate them and examine them thoroughly, they should be submitted to a current of water, of acetic acid, or of ammonia. These fluids dissolve the red, without affecting the white globules.

Whatever the nature of these latter may be, I have never observed them in the blood in circulation. It has been supposed that they are nothing more than minute fragments of fibrine, which adhere to the object-glass in consequence of their coagulating. M. Letellier affirms, that if red globules be left in a vessel, white ones may be seen to gravitate to the bottom; this militates in favour of their being formed of fibrine. I have never seen bodies of this kind in the blood of either reptiles, birds, or fishes.

I have slightly alluded to the existence of other globules in the blood, cognisable by their mammillated, raspberry-like look. Some persons are of opinion that these have no real existence, and that their supposed presence depends on an optical illusion, due to the iridescent appearances produced when two plates of glass are not in immediate contact. There is another kind, of much smaller size, and of which I discovered a large quantity in a subject affected with albuminuria; they appear to belong more particularly to the lymph and chyle.

I shall conclude this lecture by perform-

ing two experiments, and by relating to you the particulars of a very interesting case which I recently met with at the Hôtel Dieu. One of these experiments will consist in injecting some blood from a mammiferous animal into the circulating system of a bird; the other in introducing some reptile's blood into the veins of one of the mammalia.

I have had the temperature of the injection for the first experiment raised to 42° centigr. The jugular vein of this goose has been laid bare; I now make a small incision in its coats, to allow of the introduction of this little silver syringe. Although I push the piston very gently the movements of inspiration become more and more violent; indeed, animals of this class suffer extremely from such experiments as these. I have now filled and emptied the syringe three times into the animal's veins; that is, I have injected about four centilitres, an enormous quantity, when one remembers that birds have, comparatively speaking, much less blood than the mammalia.

The dog now laid on the table is the one that has already had two injections of frog's blood into its veins; it does not appear to suffer much inconvenience from them; after we part I shall have a new dose introduced. The results of both these experiments shall be made known to you in my next lecture.

The case to which I propose now, in a few words, to direct your attention, is that of a little girl of ten years of age, who was placed in my wards about a month past with an organic affection of the heart, according to the hospital register. On examining her I found that the first sound, that produced by the shock of the point of the organ between the fifth and sixth intercostal spaces, either did not exist at all, or was masked by a very loud and prolonged friction sound. In spite of all the laborious research that has been bestowed on the diseases of the heart, the diagnosis of them is not always easy; in the case I speak of, for example, the friction sound might have been the effect of contraction of the aorta; but I felt disinclined to adopt that opinion, because the pulse was full and strong; the only other condition of serious import presented by the patient was a slight bluish tint of the lips. Several days had passed, and the patient was rather in a satisfactory state; she was even preparing to leave the hospital, when, almost quite suddenly, her whole body presented the certain marks of well-characterised cyanosis: in four and twenty hours she had ceased to live. The autopsy, carefully performed, brought the following state of things to light, as you will have an opportunity of seeing for yourselves by-and-by. The aorta and semilunar valves are perfectly healthy; here, then, no cause existed for the friction sound we had heard during the patient's life. But the pulmonary artery presents a very marked state of disease; it is extremely contracted; its greatest diameter scarcely measures two and a half, or three lines. Now, as direct experiment teaches us that whenever a liquid passes suddenly from a large to a smaller vessel a friction sound is produced, we can have no difficulty in comprehending how the sound in this girl was caused. It is to be presumed, that the limited width of the pulmonary artery was here, to a certain extent, a congenital defect, for the vessel has only two, instead of three sigmoid valves.

In addition to this contraction of the pulmonary artery, you observe that there is an opening at the upper part of the septum of the ventricles, measuring two lines by three. It is probable, however, that the quantity of venous mixed with arterial blood was very small, or the cyanosis would have appeared sooner and been more highly developed. I am strengthened in this opinion by the fact, that as the ventricles dilated and contracted at the same moment, the liquid in each cavity must have acted as an obstacle to the passage of the other into the adjoining ventricle. The right ventricle has, in this case, almost as thick walls as the left. The septum between the auricles presented a similar opening which, for the same reasons as I have just stated, is not likely to have led to intermixture of the two kinds of blood. I think I may affirm, that death has here been the result of gradual contraction of the pulmonary artery.

CASES OF

LONG-CONTINUED CONTRACTION

OF THE

LOWER EXTREMITIES,

FROM AN AFFECTION OF THE SPINE.

By R. A. STAFFORD, *Esq., Surgeon to the St. Marylebone Infirmary, &c.*

CASE 1.—Jan. 1833. Mary Kean, ætat. 20, a slight-made girl, and of small stature, was admitted into St. Marylebone Infirmary with complete contraction of the right lower extremity, and contraction of the third and little finger of the hand of the same side. She possesses the full power of feeling in both limbs, but the knee-joint is firmly bent and stiff; the calf of the leg rests upon the back part of the thigh, and the heel presses closely upon the nates, the foot being turned backwards. She states that she has been the subject of epileptic fits ever since her infancy; that about two years ago she was seized with violent pain in the spine in the lumbar region; pain in the hip and knee;

and extreme pain along the course of the ischiatic nerve, accompanied with considerable tenderness and increased sensibility of the whole limb. At this period the lower limb became gradually and firmly contracted, and no means employed could prevent it. Some little time afterwards the two last fingers also became contracted, and thrown across the two first on the hand of the same side. She was admitted into one of the London hospitals, where the symptoms became mitigated, but the limbs remained the same.

For the last two years, excepting the contraction, she has continued pretty well, but at present she is suffering from her original symptoms. The pain in the back, hip, and knee, is extremely acute; and the tenderness and sensibility of the whole limb are so great that she cries out at the slightest touch, and she cannot even bear the weight of the bedclothes upon it. On tracing and pressing upon the ischiatic nerve, it is extremely painful and tender. Her pulse is quick; fever high; tongue furred; skin hot and dry, and thirst great. She has constant restlessness and sleepless nights.

These symptoms continued, being more or less violent, for three or four months; and the treatment adopted was frequent abstraction of blood from the lumbar region by cupping; the application of leeches along the course of the ischiatic nerve; the exhibition of antimonials, opiates, and aperients, as often as required, and according to the necessity of each remedy. When the chronic stage commenced counter-irritants were employed and frequently repeated, both on each side of the spine and along the course of the ischiatic nerve, in the form of blisters and the tartar emetic ointment. Lastly, issues were made and kept open for a considerable time on each side of the spine in the lumbar region.

When the inflammatory symptoms had entirely subsided, the pain in the back had ceased, and the tenderness of the limb had diminished, an attempt was made to unbend the contraction, and bring the limb into its natural state. To effect this purpose small thin pillows were first introduced between the calf of the leg and back part of the thigh, drawing them as near as possible to the flexure of the knee-joint. At first this caused extreme pain, and occasioned several attacks of epilepsy. By degrees, however, the patient was able to bear the treatment. Thicker pillows were then employed, and, as before, occasioned great pain, but which, after the limb was accustomed to them, subsided. When the pillows had been worn for some time (six weeks or two months) the contraction had relaxed sufficiently for a machine to be introduced between the two limbs, and which is so contrived that, by turning a screw, the leg can be gradually extended. It consists of two splints joined by a hinge at the flexure of the knee-joint,—one resting on the back part of the thigh, and the other on the calf of the leg; a bow made of iron, the extremities of which rest upon each splint, and through the centre of which passes a screw, which is also attached to the angle at the junction of the splints. A nut (as it is termed) is turned on the screw, and presses on the centre of the bow; consequently, when the centre is pressed upon, the two extremities extend the splints.

With this machine the limb was gradually extended, inch by inch, until at length the leg and thigh were as straight as natural. During the progress of the extension friction was employed, and the knee was frequently steamed. The limb was then exercised, and after the period of seventeen months from the beginning of the treatment to the termination of it, the patient was discharged from the hospital cured. The fingers likewise were gradually extended, and returned to their natural use and position.

This case appears to me to be one of great interest, inasmuch as it points out,—First, the nature of these affections; secondly, the treatment necessary to be employed; and lastly, that however firm and complete a contraction of this description may be, yet by perseverance and care, and the gradual and equal employment of extension, the limb can be restored to its healthy action.

That the spine in this case was the seat of the disease is shown by the symptoms; the acute pain in the back, the pain and tenderness of the ischiatic nerve, the pain in the hip and knee, the extreme sensibility of the whole limb, and lastly, the firm contraction of the leg, are all of them symptoms denoting some affection of the medulla spinalis. It appears probable that that portion of the medulla or cauda equina, from whence the right ischiatic nerve derives its origin, was either inflamed or in a morbid condition, and thus the painful affection and contraction of the limb were produced. The ring and little finger of the hand of the same side being contracted also shows that the origin of the ulnar nerve was similarly affected. Independently, however, of the symptoms, the benefit derived from the treatment is another proof that the disease was situated in the spine.

Case 2.—Ellen Sullivan, æt. 25, a stout, plethoric young woman, of a strumous habit of body, fell down upon her right knee and injured it. She was admitted into St. Marylebone Infirmary, under my care, in February 1835. The synovial membrane was inflamed, and there was considerable enlargement of the joint from the effusion of synovia. Blood was frequently abstracted from the knee by cupping and leeches; counter-irritants were employed; and in six

weeks, or two months, the joint had so far recovered that she was on the point of being discharged from the hospital. The day before the one fixed upon for her departure, however, she was again seized with excruciating pain in the knee, accompanied with pain in the back in the lumbar region. She lost blood from the part by cupping as before; the knee was constantly fomented; and a brisk aperient administered. On the following morning she was no better; the pain was almost insupportable; and, although a splint was placed under the joint to prevent it, yet contraction of the knee had commenced. The pain in the back continued; there was extreme tenderness along the whole side, from the lumbar region down the ischiatic nerve to the foot; and the pain in the knee was so great, that she could neither sleep nor remain quiet a minute together. Her pulse was very quick, averaging from 120 to 130 in the minute; fever high; tongue furred; countenance flushed, and expressive of excessive pain. She was cupped largely from the loins; numerous leeches were applied to the knee and along the course of the ischiatic nerve; fomentations were employed; and purgatives and opiates were administered as often, and in such doses as the symptoms required. The symptoms, however, continued, appearing sometimes to yield to the treatment, and then recurring in a greater or lesser degree. The knee during this time had gradually, but rapidly contracted, and no means employed could prevent it; indeed, any opposition to it, such as using a splint, &c., produced such extreme agony, that it would have been highly imprudent to have resisted it. The calf of the leg at length rested on the inside of the thigh, and the heel on the nates. The knee itself was but slightly swollen, but the skin covering it was tense and shining, and of a scarlet hue.

The acute symptoms lasted two or three months, when they gradually abated. Counter-irritants were now employed on each side of the spine in the lumbar region, along the ischiatic nerve, and on the knee-joint. These were frequently repeated, with now and then loss of blood from the loins and the affected parts, as required. Little amendment took place for two or three months more; during this time every description of medicine which appeared likely to be attended with benefit was administered, but with little effect. To enumerate some of them, without entering into particulars, I may mention carbonate of iron, iodine, veratria, morphia, quinine, hydrocyanic acid, solution of arsenic, the mineral acids, and tonics of every description, &c. But little relief was obtained from any of these remedies. The occasional loss of blood from the affected parts, counter-irritants, with the administration of opiates, and latterly the employment of mustard cataplasms more particularly, were the only means which appeared to be of any service. Under these remedies and constitutional treatment, after a period of eighteen months, the acute symptoms subsided; the pain in the back and the pain and morbid sensibility of the knee and limb abated. Means were now employed to restore the limb to its natural state; small thin pillows were at first introduced between the contraction of the leg upon the thigh, and their thickness was gradually increased. At length the machine used in the last case was applied; the limb was extended until it was perfectly straight; and by steaming, friction, and exercise, the patient was enabled to use it with perfect freedom.

This case so nearly resembles the last, that but little comment upon it is required. It may be observed that the knee was the original part affected, but that the spine was also implicated is shown by the pain in the back, the pain along the ischiatic nerve, the extreme morbid sensibility of the whole limb, and the inordinate contraction of the leg. The treatment also proves the same. After the recovery of the knee in the first instance, and when the pain returned in it again, but little benefit was derived from treating the joint alone. When, however, treatment was directed to the spine and the ischiatic nerve by the frequent abstraction of blood, counter-irritants, &c., the patient began to amend, and ultimately recovered.

Case 3.—A young lady, æt. 18, of a strumous habit, fell into bad health. She first found that she had partially lost the power over her legs; she then was seized with violent spasms and involuntary twitchings of the lower extremities. At length these spasms increased, and became so confirmed that the legs were half drawn up in a semi-flexed position, and would remain so, unless they were put down again by another person. At this period of the complaint the case was considered as chorea, and treated as such. No improvement, however, took place; the spine, therefore, was examined, and it was found that five or six vertebræ were diseased, and that there was angular curvature in the dorsal region of the spine, beginning at the fourth dorsal vertebræ. The friends of the patient would not consent to any treatment, consequently the curvature increased, and the contraction of the legs likewise. She remained in this state for a few months, when she came under my care. I found her as above described, and my first object in the treatment was to get the limbs back to their natural position. Leeches were frequently applied to the back, and counter-irritants employed. Under this treatment she improved, some consciousness of feeling in the legs returned, and the contraction became sufficiently re-

laxed to enable me to place her on one of Mr. Earle's beds; but if the limbs were touched or moved they immediately retracted. After she had been under my care for a twelvemonth, improving in health, and there being some slight amendment both in the power of feeling and of motion in the legs, her friends became extremely impatient at the tediousness of the case; they made an attempt, without my consent, to get her up; employed friction, and exercised the limbs. The spasms returned in a violent degree; they extended on to the arms and all over the body. At length she became comatose, and in this melancholy state died. No examination of the body was made.

THE LINEAGE OF ENGLISH PEERS OF ANCIENT TITLES

EXHIBITED BY MEANS OF DIAGRAMS.

By T. R. EDMONDS, *Esq.*

THE history of the heads of families who have, for many generations, filled the highest and most conspicuous stations among the people of this country, is a subject of considerable attraction, and it is hoped that the present attempt to facilitate the acquisition of one of the most interesting branches of such history will be favourably regarded by the public. The names of English peers are interwoven with the more remarkable events in the history of the English nation, some of these peers bearing titles which have been transmitted to individuals, their descendants, highly distinguished in the present generation; there arises thence a corresponding desire to know the lineage and other particulars in the history of these peers. In the historical accounts of the English Peerage will be found detailed the chief incidents which marked the life of each peer. From such histories, by extracting the dates of birth, accession, and death of each peer, I have been enabled to estimate the law of mortality of peers, which has been published in THE LANCET. (Feb. 10, 1838). This law of mortality may be regarded as a new and not unimportant addition to the previous history of the English Peerage. In the histories of the Peerage are recorded, also, the degrees of consanguinity connecting together peers and their predecessors in the same title; it is to a new mode of exhibiting this relationship that the attention of the reader is now invited. In the course of investigating the law of mortality referred to, great benefit was found to accrue from the use of genealogical diagrams constructed for the purpose of obviating defects in the record of dates of birth of peers. By the help of these diagrams (now presented) the degree of consanguinity connecting together any two peers, however remote from each other, who had inherited the same title, was readily ascertained. Without the help of these diagrams it would have been difficult to obtain any clear and distinct idea of the course of succession, and the mutual relationship between the several individuals through whom the title had been transmitted. In the ordinary written descriptions of the line of descent of any family title, the relationship of any peer is stated with regard to his immediate predecessor only (as son or brother, commonly); the relationship between his predecessor and the latter's predecessor is to be sought for in some other page of the book. In so describing the line of descent, no more than two links of the chain are presented to the view at one time. By means, however, of the diagrams annexed the reader will be able to perceive, at a glance, the whole line or chain of descent, the form it has assumed, and the connection between any two links adjoining or remote. He may perceive how many generations have intervened between the first and the existing peer of any title; he may also perceive how many elder branches have become extinct through failure of male heirs. These diagrams will probably be found to convey readily, and perspicuously, without demanding any strain of the attention, a large amount of interesting information respecting the genealogy of the chief of the English nobility, which has hitherto been almost inaccessible to the general reader.

In the annexed diagrams are represented the lines of descent of 74 titles in the English Peerage. In this number are included all existing peerages of any considerable antiquity, or such as remain after having endured more than four generations, or more than 133 years. Similar diagrams applied to the illustration of the shorter lines of descent of modern peerages, would be of comparatively inferior value. The smallness of the number (74) of existing English peerages, whose origin is of any considerable antiquity, is calculated to excite surprise. In the number just mentioned are included three or four peerages which have recently become extinct, but this excess is compensated by three or four existing peerages having been excluded by reason of some defects which rendered them less valuable in the investigation of the mortality of peers, for which investigation alone, these diagrams were constructed, without the entertaining (until lately) of any intention to offer them to the public. The oldest of the English peerages had endured through 18 generations; the least ancient of the English peerages included in the present list, had endured through four generations in 1838, to which year the statements in the diagrams extend.

It may be proper now to say a few words in explanation of the mode of construction

of the accompanying diagrams. At the head of each diagram, the place of the first peer who possessed the title is denoted by the number 1, written in the square at the upper and right hand corner. If the title has descended successively to the eldest surviving son of each peer, such descent will be indicated by the numbers 1, 2, 3, 4, &c., placed in squares immediately following each other in a vertical direction, or in the direction of a line drawn from the top to the bottom of the page. An example of this kind of descent is found under the title of the Duke of Devonshire. If a peer should have his grandson for his next successor, the son having died during the lifetime of his father, such fact is here represented by writing the Christian name of the son in the square intermediate between the squares bearing the father's number and the grandson's number. An instance of this kind occurs in the line of the Marquisate of Salisbury, the second peer having been succeeded by his grandson, the third peer. In the case of any other ancestors of peers, who, being members of younger branches, never possessed the title, their places are likewise indicated by writing their Christian names in the appropriate squares.

When a branch becomes extinct, and the title passes to the brother of the previous possessor, this occurrence is represented by placing the number of the new peer in the square next to the left of that of his brother, and in the same horizontal line; the number of the peer who was the father of these brothers is also written again, and placed immediately above that of the second of his sons who inherited the title. The number of each peer is repeated (always in the same horizontal line) as many times as there are elder branches of which he was the origin. In case the head of such branches should not have possessed the title, the Christian name is repeated. Proceeding in this way, every removal or step in a vertical direction will indicate the interval of one generation; whilst every removal in a horizontal direction will indicate the extinction of one branch of the family. In some few instances the titles have descended through females, which cases are marked in the present diagrams by placing the appropriate numbers in parentheses.

For the purpose of more easily reckoning the number of generations through which any title has passed, a marginal vertical column of numerals has been added on the left side of each page, to denote the generation to which the peers in any horizontal line belong; the first peer of each title being placed opposite the number 1 in this column, or being assumed to belong to the first generation. With the view of ascertaining the average length of a generation, the most ancient date of birth which could be found, and the most recent date of birth of peers in each line of succession, have been compared, and are placed at the foot of each diagram. The result of this comparison and inquiry is, that the average interval between the birth of a peer and that of his successor in the next generation is 31¼ years. The observed interval in any case between the birth of a peer and that of his eldest surviving son, rarely differed more than seven years from the average period just mentioned. Out of all the cases observed (more than 200 in number) no instance was found of a greater interval than 50 years between the birth of a peer and that of his eldest surviving son.

19, Regent-square, February, 1839.

Note.—To explain the tables by an example, let us take the following:—

Generation.	DORMER, B.					
1	.....	....	...	1	1	1
2	.....	....	...	Robt.	Anth.	Will.
3	.....	....	..	Chas.	Robt.	2
4	.....	....	5	5	4	3
5	7	....	7	6		
6	John	8	8		.....	.....
7	11	10	9	.....	.....	.....

The first peer had three sons, Robert, Anthony, and William, all of whom died before their right to the title accrued.

The second peer was the son of William, and grandson of the first peer.

The third peer was the son of the second peer.

The fourth peer was the son of Robert, the son of Anthony, the son of the first peer.

The fifth peer was son of Charles, the son of Robert, the son of the first peer.—(The same numbers, *e.g.* 5, 5, and 8, 8, which are repeated from right to left on the same line represent the same individual.)

The sixth and seventh peers were sons of the fifth peer.

The eighth peer was son of the seventh.

The ninth and tenth were sons of the eighth peer.

The eleventh was son of John, the son of the seventh peer.

The third, fourth, and fifth peers all belonged to the same generation, being the fourth, inclusive of the generation to which the first peer belonged.

The ninth, tenth, and eleventh peers all belonged to the seventh generation.

TABLE *showing the Form of the Line of Succession to each of* 74 *Ancient English Peerage Titles, the Number of Extinct Branches, and the mutual Relationship of all the Occupants of the same Title.*

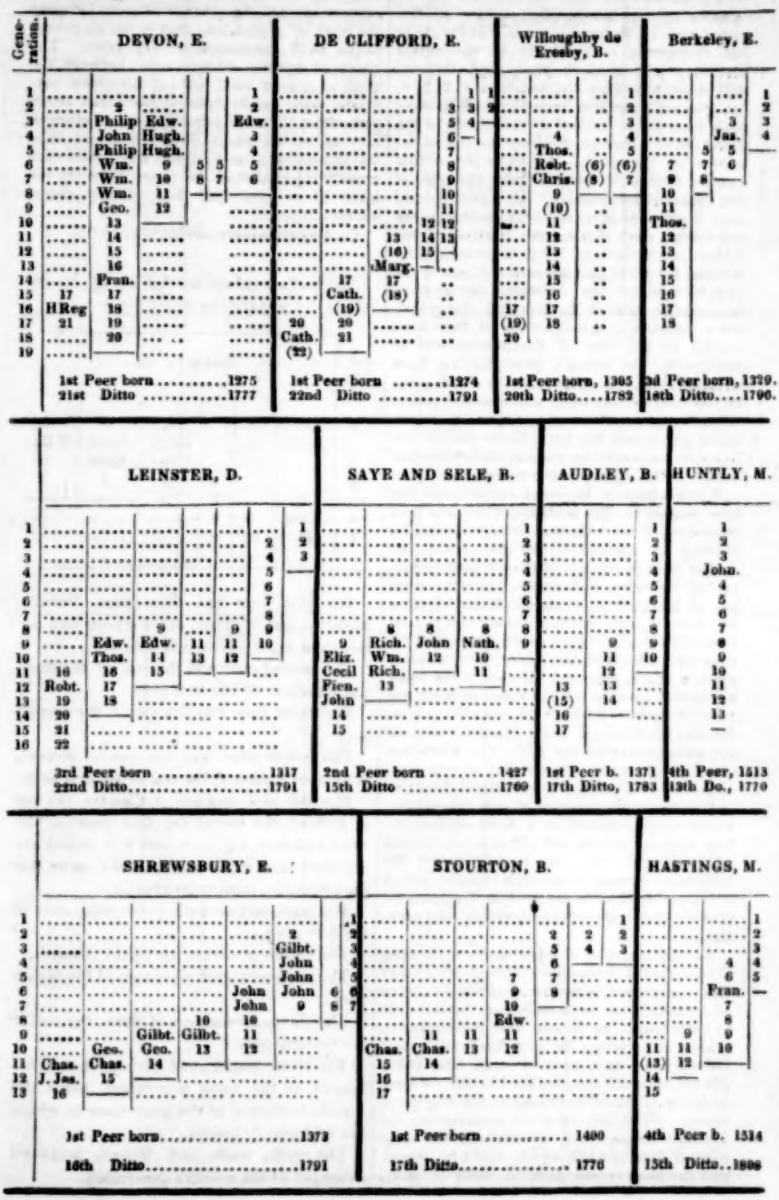

TABLE *showing the Form of the Line of Succession, &c.—(continued.)*

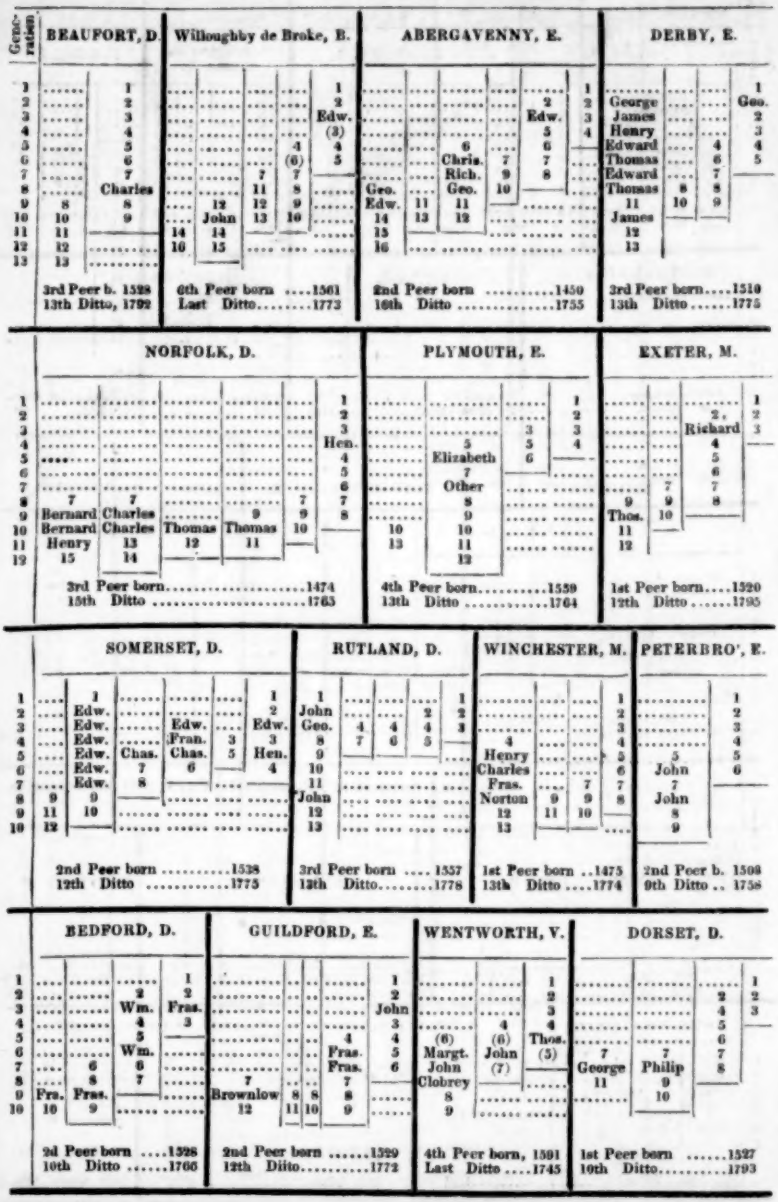
Generation
BEAUFORT, D.
1 2 3 4 5 6 7 Charles 8 9 10 11 12 13
3rd Peer b. 1528
13th Ditto, 1792
Willoughby de Broke, B.
1 2 Edw. (3) 4 5
4 (6) 7 7
11 8
12 9
John 13 10
14 14
10 15
6th Peer born 1561
Last Ditto.....1773
ABERGAVENNY, E.
1 2 3 4
2 Edw. 5 6 7 8
6 Chris. Rich. Geo. 7 9 10
Geo. 11 11
Edw. 11 12
14 13 12
15
16
2nd Peer born 1450
16th Ditto 1755
DERBY, E.
1 Geo. 2 3 4 5
George James Henry Edward Thomas Edward Thomas 11 James 12 13
4 6 7 8 9 10
3rd Peer born....1510
13th Ditto.....1775
NORFOLK, D.
1 2 3 Hen. 4 5 6 7 8 9 10 11 12
7 Bernard Bernard Henry 15
7 Charles Charles 13 14
9 Thomas Thomas 12 11
7 9 9 10
3rd Peer born.....1474
13th Ditto 1765
PLYMOUTH, E.
1 2 3 4 5 6 7 8 9 10 11 12 13
3 5 Elizabeth Other
2 3 4
10 11 12
4th Peer born.....1559
13th Ditto 1764
EXETER, M.
1 2 3 Richard 4 5 6 7 8 9 10 11 12
2 7 9
1st Peer born....1320
12th Ditto 1795
SOMERSET, D.
1 Edw. Edw. Edw. Edw. Edw. 9 10 11 12
1 Edw. Edw. Edw. Fran. Chas. 7 8
2 Edw. 3 Hen. 4
3 Chas. 6
2nd Peer born 1538
12th Ditto 1775
RUTLAND, D.
1 John Geo. 8 9 10 11 John 12 13
1 4 7
2 4 6
2 4 5
3
3rd Peer born 1557
13th Ditto.....1778
WINCHESTER, M.
1 2 3 4 Henry Charles Fras. Norton 12 13
5 6 7 8
9 7 11 10
9
1st Peer born ..1475
13th Ditto 1774
PETERBRO', E.
1 2 3 4 5 6 John 7 John 8 9
5
2nd Peer b. 1508
9th Ditto .. 1758
BEDFORD, D.
1 2 Wm. 4 5 Wm. 6 7 Fra. 10
2 Fras. 3 Fras. 9
6 8
2d Peer born 1328
10th Ditto 1766
GUILDFORD, E.
1 2 John 3 4 5 6 7 Brownlow 12
4 Fras. Fras.
8 8 11 10 9
2nd Peer born 1529
12th Ditto.....1772
WENTWORTH, V.
1 2 3 4 (6) Margt. John Clobrey 8 9
4 (6) John (7)
Thos. (5)
4th Peer born, 1591
Last Ditto 1745
DORSET, D.
1 2 3 4 5 6 7 8 George 11
7 Philip 9 10
1st Peer born 1527
10th Ditto.....1793

Table showing the Form of the Line of Succession, &c.—(continued.)

Generation	NEWCASTLE, D.	Devonshire, D.	Salisbury, M.	Pembroke, E.	Arundel, B.
1	1	1	1	1	1
2	2	2	2	2	2
3	Edward 3	3	Charles	3	3
4	Francis 4	4	3	4	4
5	6 Edw. 5	5	4	5	5
6	7 7	6	5	6 7	6
7	9 8	7	6	8	Jas. 7
8	10	8	7	9	9 8
9	11	9	8	10 11	10
	1st Peer born..1312 11th Ditto 1765	1st Peer born, 1559 9th Ditto....1790	1st Peer born, 1563 8th Ditto....1791	1st Peer born, 1560 12th Ditto ..1791	1st Peer born, 1560 10th Ditto ..1783

	NORTHAMPTON, M.	ANGLESEY, M.	HEREFORD, V.
1	1	1 1	1 1
2	2	3 2	Edward Edward Rich.
3	3	4	George 5 2
4	4	5	George 6 George 6 3
5	5 5 5	Henry 6	Vaughan 8 Price 7 4
6	Charles Charles 7 6	Thomas 7	Arthur 9
7	9 8	Caroline Thomas	11 11 10
8	10	9 8	13 12
9	11	10	14
	1st Peer born..... 1538 11th Ditto..... 1790	1st Peer born.... 1566 10th Ditto 1768	6th Peer born.....1625 14th Ditto.....1777

	ST. JOHN, B.	PETRE, B.	TEYNHAM, B.
1	1 1	1	1
2	3 2	2	2
3	Paulet 4	3 3 3	3
4	Oliver Paulet Paulet	6 5 4	4
5	Andrew Andrew Andrew Andrew 6 5	7	5 5 5
6	10 9 8 Andrew	8	8 8 7 6
7	11 11 7	9	10 10
8	13 12	10	Fran. 11 11
9	14	11	14 13 12
	Grandfather of 8th Peer born .....1624 14th Peer born 1811	2nd Peer b. 1580 11th Ditto ..1793	1st Peer born.....1534 14th Ditto.....1768

	SUFFOLK, E.	Denbigh, E.	Sandwich, E.	Stamford, E.	Manchester, D.
1	1 1	1 1	1	1	1
2	Thos. Thos. 2 2 2	George 2	2	John	2
3	Philip Wm. 5 5 5 4 3	3	3	2 2	3
4	Chas. Crav. 9 8 6	4	Edw. Richard	John Thos.	4 4
9	Philip 11 11 10 7	5	4	4 3	6 5
6	15 14 Wm.	6	5	5	7
7	16 12	William	6	6	8
8	13	7	7	7	
	1st Peer born.....1561 16th Ditto .....1776 13th Ditto 1779	Father of 3rd Peer, 1616 7th Ditto 1796	1st Peer b. 1625 7th Ditto 1811	1st Peer, 1547 7th Do., 1765	2nd Peer, 1602 8th Ditto 1771 or 1768

	BRIDGEWATER, E.	WINCHELSEA, E.	WESTMORLAND, E.
1	1	1 1	1
2	2	Heneage 2	Francis 2 2
3	3	Heneage 3 3 3	Francis 4 4 4 3
4	4 4	7 7 6 5 Wm. 4	Henry 7 6 5
5	Henry 5 5	Edward William 8	8
6	John John 7 6	George 9	9
7	9 8	10	10
	1st Peer born .....1540 8th Ditto.....1753	1st Peer born .....1558 10th Ditto 1791	1st Peer born .....1583 10th Ditto.....1789

TABLE *showing the Form of the Line of Succession, &c.—(continued.)*

Gene-ration.	DORMER, B.	Chesterfield, E.	Buccleuch, D.	Poulett, E.	Cornwallis, E.
1	1	1	1	1	1
2	Robt.	Arthur	2	2	2
3	Chas.	Charles	3	3	3
4	5	Michael	Francis	4	4
5	7	Arthur	4	5	5
6	John	5	5	6	6
7	11	6	6	7	8
				8	9
	2nd Peer born 1610	1st Peer, b. 1584	1st Peer, 1649	2nd Peer, 1615	3rd Peer, 1655
	11th Ditto ..... 177—	6th Ditto, 1808	6th Do., 1806	8th Ditto, 1783	9th Ditto, 1778
	9th Ditto 1753				

Gene-ration.	CRAVEN, E.	WARD, B.	Townshend, M.	Bute, M.	Essex, E.
0	Robt.	1	1	1	1
1	Thos.	Wm.	2	2	2
2	Wm.	Wm.	3	3	3
3	John	Wm.	4	4	4
4	John	6	5	John	5
5	6	8	6	5	6
6	7	9			
7	8	10			
	1st Peer born 1608	4th Peer born 1705	1st Peer, 1595	3rd Peer, 1713	4th Peer, 1697
	8th Ditto 1809	Last Ditto 1817	6th Ditto 1778	5th Ditto, 1793	6th Ditto, 1757

	THANET, E.	Cardigan, E.	Carlisle, E.	Dartmouth, E.	Tankerville, E.
1	1	1	1	1	1
2	2	2	2	2	2
3	Sackv.	Fras.	3	George	3
4	7	3	4	3	4
5	8	Robt.	5	4	5
6	9	6	6	5	6
	3rd Peer born 1631	1st Peer b. 1583	1st Peer, 1628	1st Peer, 1647	1st Peer, 1618
	11th Ditto 1775	6th Ditto, 1769	6th Ditto 1773	5th Ditto 1784	6th Do., 1776

	COVENTRY, E	Graham, M.	DIGBY, E.	FALCONBERG, V.	BYRON, B.
0	0		1		Jno. Jno.
1	Walter	1		1	2 1
2	Walter	2	2 2 2	Hen. Hen.	3
3	9 Thos.	3	5 4 3	Rowl. Rowl. 2	4 4
4	10 5 5	4	Edw. Edw.	Rowl. 3	Jno. Jno. 5
5	11 8 6	5	7 6	Anth. Anth. 4	Geo Jno.
6	12 7	6	8	7 6 5	7 6
	Father of 1st Peer, 1547	1st Peer, 1612	Father of 1st Peer born 1580	1st Peer born 1577	2nd Peer b. 1605
	12th Peer born .. 1784	6th Do., 1755	8th Peer b. .. 1773	7th Ditto 1750	7th Ditto .. 1789

	Waldegrave, E.	Portland, D.	Grafton, D.	Shaftesbury, E.	Abingdon, E.	Cholmondeley, M
1	1	1	1	1	1 1	0 0
2	2	2	2	2	James 2	1 2
3	3	3	Augustus	3	3	3
4	4	4	3	4	4	George
5	5	5	4	5	5	4
6	6					5
	2nd Peer b. 1684	1st Peer b. 1648	1st Peer b. 1653	1st Peer b. 1671	1st Peer b. 1653	1st Peer b. 1662
	8th Ditto .. 1816	5th Ditto 1709	4th Ditto 1760	6th Ditto..1708	8th Ditto 1784	5th Ditto 1792

TABLE *showing the Form of the Line of Succession, &c.—(continued.)*

Generation	FERRERS, E.						ASHBURNHAM, E.		ONSLOW, E.	
									0	0
1	....	.....	.....	1	1	1	1	1	Foot	1
2	....	Lawrance	Lawrance	Lawrance	3	2	3	2	Arthur	2
3	6	6	5	4	....	....	4	.....	4	3
4	8	7	.....	.....	....	....	5	.....	5	
5	....	.....	.....	.....	....	....	6	.....	6	
		2nd Peer born 1677					1st Peer born....1625		Father of 1st Peer born 1622	
		8th Ditto 1760					6th Ditto 1797		6th Peer born ..1777	

TESTS OF ADULTERATED DRUGS.

To the Editor of THE LANCET.

SIR:—I think Dr. Thomson has fallen into some error in the way he has given sulphuric acid as a test for the adulteration of nitrate of silver. Sulphuric acid will throw down a precipitate (a "white powder") from a solution of the purest nitrate of silver that can be made, and this precipitate is not sulphate of lead, but *sulphate of silver* in the form of very minute crystals; while, at the same time no "nitrate of silver will remain in solution."

Sulphuric acid may, however, be made available as a very useful test, as follows:—Brande, Henry, Ure, and other chemists, agree that sulphate of silver is soluble in 90 parts of water at 60°, and still more soluble in hot water; while it is equally agreed that sulphate of lead is perfectly insoluble. If, therefore, the precipitate thrown down by sulphuric acid be treated with an *excess* of water (hot will be best), the sulphate of silver will be *dissolved*, and what remains may be regarded as sulphate of lead, and therefore adulteration. Every 10 grains of this, when thoroughly dried, will indicate very nearly 11 of nitrate of lead. The tests given in the Notæ Breves of the Pharmacopœia are sufficient as *qualitative* ones, and the ammonia test for the chloride of silver is very useful; but it was not so very wise to make the student test with sulphuric acid for lead, when the whole of it will have been thrown down into a scarcely soluble chloride. Whatever indication it might give of the presence of lead, it can give none of its *quantity*. As a test for the nitrate of lead, I should prefer a solution of sulphate of soda (first freed of any chlorine it may have by sulphate of silver); and when sulphuric acid is employed, it is quite necessary to remark that it generally contains sulphate of lead, and should, therefore, be diluted beforehand. It will doubtless be of the highest importance, both to the profession and the public, that the villany impressed into the service of drug-adulteration should be thoroughly exposed and deprecated; but, above all, it is desirable that the test by which the genuine article may be known, should also be well established, in order that we may not come to false conclusions of its composition, and impugn, too hastily, the reputation of manufacturers known, perchance, for their integrity and respectability. I am, Sir, your obedient humble servant,

WM. GALLARD.

Hampstead, Feb. 26.

OBLITERATION OF VEINS.

To the Editor of THE LANCET.

SIR:—In the report of a clinical lecture, delivered by Mr. Liston at the University College Hospital, which appeared in your Journal of February 2nd, I find it stated, that, in alluding to some experiments I had performed on the obliteration of veins in animals, Mr. Liston observed, "that I had found the blood in the veins coagulated sixteen hours after the introduction of the needles." I would remark, however, that not only was the blood coagulated after the needles had remained in that time (a phenomenon which, probably, takes place much earlier), but that, on killing the animal some days afterwards, the vein was perfectly obliterated, owing to the contraction of the vessel around the coagulum. Such was the substance of the remarks made by Mr. Liston; but which, owing to an error of your reporter, have been misstated.

I have not published any account of these experiments, not having had an opportunity of verifying my observations on the human

body; and it is very probable, as Mr. Liston observes, that the veins in the human subject might not be so readily obliterated as in dogs. The subject, I think, is one worthy the attention of the profession, as any proceeding which tends to lessen the irritation produced in causing the obliteration of varicose veins, must be of decided advantage, owing to the violent inflammation which often follows even slight injuries when occurring in a part which is the seat of this diseased state of the vessels.

From the result of my experiments I should conclude, that by arresting the circulation in the vessel for a few hours (twelve or sixteen); by passing a couple of needles under it and the twisted suture; and by the application of gentle external pressure, the slight degree of irritation, thus produced, will be sufficient to cause such an effusion of lymph and serum in the sheath of the vessel as to lead to its permanent obliteration. I remain, Sir, your obedient servant,

JAMES BLAKE.

University College, Feb. 23, 1839.

SUPPRESSION OF QUACKERY.

We shall, at another time, draw the attention of our readers to some judicious observations which have been recently published by Dr. Cowan, on the subject of quackery. We have at present only room for Dr. Cowan's conclusions:—

Firstly.—The first and most important requisition should be, that all quack advertisements, either through the medium of the newspapers, placards, pamphlets, or other printed channels, be declared illegal, and that both the printer and the author be liable, for each offence, to fine and imprisonment.

Remarks.—There is nothing which should startle us in this proposal, nothing which improperly infringes the liberty of the subject or of the press. Quack advertisements are equivalent to, and worse in their effects, than the personal and unqualified practice of medicine: the quack having the power of discrimination. Their necessarily injurious influence upon the public health, their flagrant falsehoods, their mercenary character, their direct violation of the whole spirit and privileges of the constituted medical bodies, at once point out their contraband nature,—their illegality. To demand their entire suppression is, therefore, to ask for the most ordinary exercise of protective power on the part of the Government; and the more we have reflected upon this subject, the more satisfied we are that every other means which can be devised will prove comparatively inert, unless assisted by the one we are now recommending.

Secondly.—We should demand the abolition of the stamp duty on medicines.

Remarks.—There is no restriction whatever in the obtaining of a stamp,—no inquiry instituted as to the properties of the article thus legalised. It is only necessary to compound a nostrum, and puff it in any way best suited to our purposes, and Government never refuses to sanction it by a stamp. We do not think that stamps were *intended* to express legislative approval, but their moral tendency upon the public mind is unquestionably to do so.

Thirdly.—That the sale of all secret medicines (unless sanctioned by a medical jury, and accompanied with statements ratified by the experience of qualified and respectable practitioners), be declared illegal, and the vendors thereof liable to a fine for every offence.*

Remarks.—We need scarcely observe to professional readers that almost the whole of quack medicines are the revival of some once popular formula of our obsolete pharmacopœias, or are the recipe of some well known medical man, and which he was frequently in the habit of prescribing. There is scarcely one which has any claim to novelty, nor is there one, if we except Dr. James's fever powder, possessed of any properties which could for a moment counterbalance the evil of retaining it in a secret form; and that we could very easily dispense with this far-famed preparation, will be readily admitted by all. The whole genius of a liberal profession is and should be opposed to what is secret, and no individual belonging to it should be allowed to make a profit by any such preparation beyond the limits of his own practice. Medical bodies should be possessed of summary power to deprive of his qualification any individual who pursued this, or any other unprofessional mode of making money, or gaining notoriety.

Should an instance occur where a means has been discovered which pretends to greater virtue than any we previously possessed, let the discoverer state his claims before a medical commission, and if they are substantiated let him be rewarded accordingly; but the secret must then be divulged. And if an unprofessional person should make the same pretensions, but refuse to disclose the secret, let him only be allowed to use it after he has qualified himself for so doing, by going through the appointed course of medical studies, which can alone render him competent to judge of

* The clause between parentheses is introduced to meet the prejudices of some among our rulers, who still think that many valuable remedies would be lost to the public were all secret medicines abolished. We think it would be much better to suppress them altogether.

its real efficacy, or to exercise any proper discretion in its administration.

Fourthly.—That all establishments for the practice of medicine, or the application of remedial means, unless conducted by properly qualified medical men, be at once put down, and their originators rendered liable to fine and imprisonment. The necessity for this is too apparent to need comment.

Fifthly.—That any individual guilty of practising, or of attempting to practise, without a legal qualification to do so (always excepting cases of gratuitous administration of remedies when medical advice cannot be procured), be, *ipso facto*, liable to punishment, the penalty to increase with each repetition of the offence.

Remarks.—To render legislation cheap and speedy, and to distinguish who is and who is not entitled to practise, the action might be simply founded upon the possession of a certificate or diploma from some recognised University, or, as has been lately recommended, upon the fact of the individual being registered or not in an office appointed for that purpose. If this last regulation was cautiously enforced, it would be less liable to be evaded than the first.

Sixthly.—Let a Medical Censor be appointed, with all due publicity, in every district of convenient dimensions, whose bounden duty it shall be, either from personal knowledge, or from information received from others, to enforce the penalties attached to the infringement of medical laws.

Remarks.—This provision is important, for were the power vested in the members of the profession generally, they would scarcely ever be exerted, since few would feel disposed voluntarily to incur the trouble and unpleasantness attached to such interference.

Seventhly.—Let all penalties be inflicted by the local magistrate; and let the offence be simply grounded upon unqualified practice, to be determined by the plan we have already mentioned.

Remarks.—Simplifying the offence, and rendering the legal process unexpensive and speedy, is so essential, that no enactments will prove practically efficient which cannot be thus enforced.

We do not consider the above as a complete code of anti-empirical regulations, but as including the leading and more essential restrictions; and though we are quite ready to admit that quackery, in some form or other, will continue to prevail in spite of the best contrived preventive legislation (because originating in causes too deep for human laws to uproot), yet we are equally convinced that we have it in our power to control, most materially, the extent of its operations, and to reduce its immoral and devastating influence to within comparatively very narrow limits.

The course to be pursued by the medical profession is to unite in *unanimous petitioning*. Each town and district should send its separate petition, and this should be forwarded to the respective members of the locality, requesting their personal support. The plan usually adopted of sending all petitions to one or two who are interested in medical questions, is materially to diminish the general effect upon the House, and the members, individually, feel no interest in the subject, or, at least, each concludes that its advocacy is entrusted to another. The number of the petitions is of more importance than the number of signatures, though the more numerously they are signed the better. They should, under all circumstances, be forwarded. It would be also very desirable that they should be simultaneously prepared in time for presentation early in the next session.

*** In giving publicity to the above propositions of Dr. Cowan, we wish it to be understood that we publish them simply as the ideas of their author; our own sentiments upon this important subject shall be made known upon some future occasion.

WESTMINSTER MEDICAL SOCIETY.

Saturday, March 2, 1839.

Dr. Chowne, President.

THE VESICULA ALBA.—MALARIA.

Mr. Streeter exhibited to the Society a beautiful preparation, showing the vesicula alba, in an abortion at probably about the seventh week; and stated that Velpeau remarks that he had only found it, in anything approaching to a normal state, in thirty out of two hundred abortions prior to the third month. Some observations were made on the facility or difficulty with which the vesicle in question could be dissected, after which the subject of malaria was again brought forward.

Dr. A. Thomson repeated the observations he made at the last meeting, and adduced as evidence of the presence of free hydrogen in the air during the prevalence of fogs, the fact that ammonia at that time was generated in places where at other times no such gas was produced. Regarding malaria, much, no doubt, depended upon moisture, but certainly not all; this was proved from the circumstance of people in many moist situations, such as isolated spots of ground near the ruins of Babylon, in the midst of a marsh, as stated by Mr. Ainsworth, never suffering from any malarious diseases. He (Dr. T.) had no doubt but that malaria was a gas, but of its nature we were completely ignorant. In stirring

up stagnant pools and ditches carburetted hydrogen was evolved; this was no doubt injurious; M'Culloch had, however, said that this gas could not be malaria, because it would not of itself produce the diseases which malaria generates. But this was no argument against the identity of the two agents, because carburetted hydrogen when applied directly would destroy life, before time were allowed for any of the effects in question to be brought on.

Mr. VERRALL had practised for many years in a very marshy district in England, where malarious fevers were very prevalent. He believed that malaria did not consist of one gas, but of many, and he was led to this belief from observing that each season had its peculiar type of fever. Thus, a whole village would at one time have quartan fever, or remittent, and not a case of the other forms of fever would be present. Now, if the cause was not different at one time from another, why should not there be a variety in the forms of the fever, instead of its being restricted to one type during an entire season, among the whole of the persons affected? He believed that the poison which produced intermittent and remittent fevers was essentially different from that which generated continued fever; the former he considered having their origin from vegetable, the latter from animal, decay.

Dr. STONE said that the late Dr. Fletcher had advanced an ingenious theory respecting malaria. The opinion of that eminent physiologist was, that the poison did not arise from animal or vegetable decay, but was, in fact, a secretion from the living vegetable in a state of disease consequent upon the locality in which it grew. He thought that as many diseases might be communicated by a vitiated secretion, such, for instance, as small-pox, measles, and other like disorders, a diseased vegetable secretion might give rise to malarious fevers; and he considered that draining a country was beneficial to the health of the inhabitants, from the source of the disease in the vegetable being removed. Dr. Fletcher also attributed the variety which is observed in the diseases of marshy districts to the different kind of diseases under which the plants laboured, producing a variety of morbid secretions.

Dr. G. BIRD said that in inquiries like that which had been entertained respecting the nature of malaria, we gained something from negative facts. He (Dr. B.) had at one time believed that carburetted hydrogen and malaria were identical, from having on one occasion, as he conceived, caught a severe ague from collecting a quantity of the gas in question from stagnant pools and ditches. He had subsequently however, abandoned this opinion from finding that in an extensive coal district, in the county of Fife, in which carburetted hydrogen was constantly being evolved in large quantities, no case in any way resembling ague had occurred for a period exceeding twenty years; malaria was, therefore, not carburetted hydrogen, although it might be in some way connected with it. Regarding the theory of Dr. Fletcher, if correct, malaria ought to be most prevalent in places where there was the largest quantity of vegetables; this he probably would have said was the case; but he (Dr. Bird), would point out a spot near which no vegetable grew, and yet malarious fevers prevailed to a frightful extent; he alluded to the neighbourhood of the Fleet-ditch, which was full of putrescent animal matter. The idea stated by Mr. Verrall, that vegetable decay produced ague and remittent fevers, and animal putrescence those of a continued type, had, at least, this fact in its favour, viz., the prevalence of the former in marshy districts, and of the latter in large cities. Professor Rossi, of Turin, had collected some blood from patients labouring under a fatal petechial fever. This blood he placed in a bladder, through which a wire connected with a galvanic battery was passed into a vessel containing water; the water was decomposed into its constituents, oxygen and hydrogen, but there was an evolution of a third gas, which he attributed to the passage of the wire through the blood, and which he considered had something to do with the production of the fever. He (Dr. B.) might also mention another fact in connection with malaria. In a marshy district of Norfolk, in which intermittent fever was remarkably prevalent, there were certain spots of a few acres of ground in extent, in the very heart of the district, where the disease seemed to exert no power; these spots were occupied by skin-dressers, a decomposition of animal matter was, consequently, always going on. The proprietor of one of these factories had told him (Dr. Bird) that if any of his men went beyond the bound of the premises before seven o'clock in the morning, when the fogs were prevalent, they were sure to contract ague; they might, however, pass the bounds in the after-part of the day with impunity.

Dr. BUREAUD inquired what was the reason that the people of London, at one time so subject to malarious fever, were now comparatively free from it?

Dr. L. STEWART said that, although intermittent fever was much less prevalent in the metropolis now than in former times, yet in 1829 there seemed a tendency to a return of this liability, as, in that year, almost every disease assumed a periodic character, and he (Dr. S.) who had lived unhurt in many marshy districts, contracted a very severe ague during that year. On what cause did this liability depend?

The debate was then adjourned.

MEDICAL SOCIETY OF LONDON.

Monday, March 4th, 1839.

Mr. BRYANT, President.

THE Society was occupied this evening with a discussion on the causes and pathology of hysteria, in which a number of anomalous cases were related, as depending upon that peculiar state of the nervous system which comes under the above denomination, and a variety of opinions offered on this often-discussed subject.

The President, on leaving the Chair, his term of office having expired, made a short address to the members, and retired amid much cheering.

ROYAL MEDICO-CHIRURGICAL SOCIETY.

THE Society held its anniversary meeting on Friday, the 1st of March, for the election of officers for the ensuing session. Sir B. Brodie was elected President in the room of Dr. Bright, and Dr. H. Lee, Secretary, in the place of Dr. Clendinning, resigned. On retiring from the chair, Dr. Bright delivered a retrospective address, in which he congratulated the members on the prosperous state of the Society, in regard to its funds, admission of members, and the value of the papers read during the last session. For the future the papers would be collected by the Secretary at the end of each session, for the purpose of immediate publication; the members would not, therefore, have to wait as they had previously done, a long period before the communications were laid before the public. The Society had to lament the death of two of its Fellows, both cut off in the prime of life, and in the active pursuit of their profession —they were Dr. Sims and Dr. Fergus. Dr. Sims was remarkable for the energy with which he carried out those plans which he considered would be beneficial to the profession, and, whatever difference of opinion might exist as to the value of these projects, every one must have admired the energy with which they were pursued. Dr. Sims was for several years physician to the Marylebone Infirmary, and had published several papers in the Society's "Transactions." He died from a fever which he contracted in the wards of the Marylebone Infirmary, his health having been previously undermined from a dissection wound which he received some years ago. Dr. Fergus was a younger man than Dr. Sims, but not less ardent in the pursuit of that department of the science of medicine to which the bent of his mind inclined him: the statistical and pathological branches. Only one article, however, with his name attached had appeared, and that consisted of a statistical chapter in Sir J. Clark's work on Phthisis.

A System of Practical Surgery; with numerous Explanatory Plates, the Drawings after Nature. By JOHN LIZARS, Professor of Surgery to the Royal College of Surgeons, Edinburgh, &c. &c. Part I., 1838.

As part of a system of practical surgery, the volume before us is far from being perfect; as a class-book in the hands of students attending surgical lectures it will be of value; to the practitioner its only recommendation will be found in the bird's-eye view which the plates give of operations, bandages, knives, tenacula, and even the ordinary cupping apparatus; and as a contribution to surgical science and literature in the nineteenth century it is but a very mediocre production.

Part I. contains but 220 pages of letterpress, and as many as 18 plates, the average number of figures in each varying from two to eleven. It embraces a history of surgery, inflammation and its consequences, dissecting-room wounds, arteriotomy and phlebotomy, ulcers, diseases of arteries, aneurysm, hæmorrhage, fractures, dislocations, gun-shot wounds, and amputations; any two of which subjects, in the most succinct treatise, could not be correctly disposed of in one half the compass of our author's entire volume. Mr. Lizars dedicates his "System of Surgery" to the President and Fellows of the Royal College of Surgeons, Edinburgh, and states that ever since he has been engaged in the duties of the professorships with which they collectively honoured him, he has felt "the want of a book containing each surgical disease, and the corresponding operation, succinctly and correctly delineated, so as to be a useful guide to the student, and remembrancer to the practitioner."

That there is room for such a book we fully agree with Mr. Lizars, but we regret to say that his volume does not fill up the hiatus; it is too superficial; the condensation he aims at has led our author into omissions of serious moment in the diagnoses of special affections, a meagre account of inflammation and its consequences, and a railway history of surgery, from the days of Patroclus and "the godlike sons of Æsculapius," down to those of Benjamin and John Bell extending over five pages! Brevity may be the soul of wit, but it is the mutilator of medical writings. In avoiding one extreme Mr. Lizars has gone into the

opposite; the 3036 columns of the "Surgical Dictionary," with all its improvements and enlargements, have made it a work almost useless to the practitioner as one of reference in emergencies, and to the student of surgery a bugbear; the several pocket volumes on surgery, condensed principally from the courses of lectures which have been delivered in the schools of medicine, are miserably deficient: it was between these two extremes, then, that Mr. Lizars should have taken his ground, and let us hope that at some future period the "System of Practical Surgery" now under our consideration will receive those improvements at its author's hands which he is so well fitted to make, and which his work stands so much in need of, to render it a boon even to the learned Professor himself. In the limited space which our author has given to his history of surgery, he has successfully shown that surgery advanced as a science only in proportion as anatomy—the foundation of all medical knowledge—was cultivated; and that when despotism, on the one hand, exercised her sway in the suppression of anatomical pursuits, or superstition, on the other, held in bondage the minds of the many, thus rendering the anatomist an object for persecution, so surely did medical *science* come to a stand, and each successive year retrograde even in a ratio much greater than it had previously advanced. Speaking of the blending of surgery with physic, Professor Lizars remarks, "that it was only at the dawn of medicine, and in the present era," being at these periods so blended, that it "became a respectable profession." And lest any doubt should be entertained of Professor Lizars' meaning, we subjoin the sentence which follows:—

"It is much to be regretted that there exists any separation, since it only tends to depreciate each, as physicians must be consulted in accidents, the province of the surgeon, and the surgeon must be acquainted with the treatment of fever supervening to operation."

Now, we differ altogether from Professor Lizars as to the respectability of the surgical profession at any time being dependent upon its blending itself with physic; whenever it did so the surgeon was looked upon by the physician as a mere tool in his hands, and if he endeavoured to emancipate himself from the thraldom of his masters, he was held up to the odium of the public, and accused of arrogance and presumption for any opinion he dared to offer beyond the mechanical exercise of his profession. No; surgery was never so triumphant as a *science* as when it stood upon its own resources; then it needed not the sombre countenance of the physician to lead its patient to a successful recovery from the disease which afflicted him; and even much less required his aid in conducting the sufferer through the constitutional disturbances which may have preceded or supervened upon an operation. A few years since one of the best medical essays which ever appeared from the pen of an observant practitioner was that entitled, "The Constitutional Origin of Local Diseases," written by Mr. Abernethy.

It was the learned contempt which the associations of physicians had for the prosecution of practical anatomy, and all such "handicraft" researches, that made the surgeon an object, in their estimation, of inferior endowments. The more, however, the surgeon persevered in such investigations, the more the *functions* of the parts he examined became manifest; and as healthy structure and function were made apparent, so were the deviations from these conditions recognised. The well-educated physician of modern times follows the example; consigning to the tomb of the Capulets the decorated cranium and the gold-headed cane, he puts forth his claims to distinction in his profession on a more solid basis. Professor Lizars truly observes:—"Lately the College of Physicians of Edinburgh have *wisely* rescinded a law which PROHIBITED their Fellows from using the lancet or the scalpel;" and although we have not followed the example of our brethren in the north, as a melancholy fact has recently shown, yet the ungodly amongst our Fellows and Licentiates who have condescended to write, no matter how incorrectly, on the heads, spines, and processes of the bones of the upper extremity, and to describe the position and vascular supply of the thoracic viscera, give evidence of that state of medical consanguinity to which we are daily arriving, and which can alone be effected by a liberal and extended education. "All, therefore," says Professor Lizars "ought to have the same elementary education, and be qualified for *every* department."

In considering the interesting and important question of medical education, upon

which Mr. Lizars has so slightly touched, we had nearly lost sight of the more immediate object of his volume.

Chapter I. opens with inflammation, which our author says "is a disease which, in a greater or less degree, follows accidents and all the operations of surgery." In former numbers of this Journal we have made our readers acquainted with the important views of Dr. Macartney on this subject, who, to our minds, has most clearly shown that accidents and operations are not necessarily followed by inflammation. It is to be presumed, however, that Mr. Lizars had not seen Dr. Macartney's work, as no mention is made of that author's opinions on this important and interesting subject. In the treatment of inflammation some judicious observations are made on the manner of applying leeches, and the method of treating them when they fall off. "When the animal falls off it should be immersed in porter for a few seconds, or in soft brown sugar, and afterwards put in clean water, which should be frequently changed for the first few days. Salt and stripping often kills them."

Next follow directions for making punctures with the lancet in inflamed parts, and how to perform the operation of cupping, both of which are illustrated by five engravings. When and where the surgeon should prefer one method of abstracting blood above another is thus stated:—"Leeches are preferable to cupping in affections of the face, the forepart of the neck, the scrotum, the penis, the pudenda, the anus; but in these parts, when inflammation is diffused, punctures with the lancet are most serviceable." At page 32 our author insists that when a vein of the left arm is to be opened, the surgeon should use his left hand, and concludes that, "therefore, he ought to be ambidexter, not only for this operation, but to be adroit in some operations on the right eye."

We have had more than one opportunity of witnessing Mr. Lizars' adroitness with his sinister member in dealing with cataract, but desirable as such dexterity is to the operator, ninety-nine surgeons out of the hundred cannot acquire it. The majority, however, have long since contrived to operate with their right hand as successfully upon one side of the body as the other, by various manœuvres, such, for instance, as Mr. Lizars himself describes when speaking of operating upon the femoral artery:—"When the left femoral artery is the subject of operation, the line of the incisions should be reversed; the operator begins at the termination of the extended line, and cuts upwards to the apex of the triangle." It is all the better, therefore, for the surgeon, as regards himself, to be ambidexter, and in the operating theatre a left-handed operation, when well performed, produces, from its novelty, a much greater effect than a right-handed one; but as regards the patient it can be of little moment.

We cannot pass by the following remark:—"Immediately on incising the skin and cellular tissue, the saphena interna vein is often brought into view, and might be mistaken for the artery; but as it runs superficial to the fascia lata, such a mistake is easily avoided." We do not know to what class of practitioners or students Mr. Lizars addresses this sentence; but the man who, in the dead or the living limb, would hesitate for a second between the saphena vein and femoral artery, ought to lay aside for ever his scalpel, as we believe no one who ever even saw a dissected lower extremity could mistake one for the other; at all events he must be a bold *chield* who would essay a great operation of this kind with such a confused notion of the position, depth, and appearance of these two vessels.

In describing luxation of the shoulder into the axilla, Mr. Lizars has omitted, among its symptoms, any mention of the lengthening of the limb, of the rotundity of the shoulder being lost, of the sense of numbness, or of the inability of the patient to bring his elbow to the side. He also describes the short muscles around the joint as being lacerated, and even some of the longer, but such severe injuries seldom occur. The supra spinatus muscle is certainly in general torn, but even this does not always take place.

We recollect having witnessed a post-mortem examination of a case of dislocation into the axilla soon after the receipt of the injury, the patient dying from other causes, and none of the muscles were torn. Our author makes short work of the other dislocations of the humerus:—

"The other directions of this luxation have all the conspicuous depression under

the acromion; and so nearly the same are the other appearances and symptoms with that into the axilla, and so similar the treatment, that it would be superfluous to describe them individually."

Consistent with brevity Mr. Lizars may think the description of the other dislocations superfluous, but that he does not describe them on account of their nearly resembling each other, is a very unsurgical excuse.

The plates, which are neatly executed, are the best part of the work. Here and there, however, the parts are out of proportion; thus, in plate 6, figure 4, the organs of generation extend halfway down the thigh; and figure 2, plate 7, which represents "an enlarged view of operations for securing external iliac and common femoral arteries," is extremely confused. In many of the figures, also, the fingers of the assistant, and even the operator himself, are placed in great jeopardy; out of many instances of this kind the most remarkable is figure 3, plate 16, which represents the "first view of amputation of the leg," letter *a* denoting "the right hand of the operator with the knife, about to make the flap;" it is rather about to cut off the left thumb of the operator himself.

The volume, taking it altogether, is exceedingly well got up, as regards paper, type, and engravings, and when it receives from the hands of its author the many additions which it so much stands in need of as a system of surgery, it will become a useful publication.

DISTINCT INQUESTS, AND CONTRADICTORY VERDICTS, *ON ONE and the SAME BODY.*

First Inquest.—An inquest was held before Mr. Gell, Coroner for the city of Westminster, and a jury, at 4 o'clock, P.M., on Monday 26th February, 1839, at the Guinea public-house, North Bruton-mews, Berkley-square, on view of the body of John Boutall, aged 52, a veterinary surgeon, late residing at 32, North-row, Grosvenor-square.

The Coroner and Jury proceeded to view the body, which lay in a bed-room, at No. 18, Bruton-mews, premises belonging to deceased, and in which he was found dead. Having returned, the following were the only witnesses examined:—

Mr. Samuel Gould.—I reside at No. 42, North-row, Grosvenor-square, and I have known the deceased, Mr. John Boutall, since the year 1828. Deceased was a veterinary surgeon, and resided at No. 32, North-row. A little before Christmas I became his clerk and book-keeper, and was in the habit of seeing him daily since that time. Deceased was a powerful large man —I may say an immense man—[Here the Coroner, interrupting the witness, remarked, "He was so; I knew him very well; he was a remarkably stout built man"], and his constitution was excellent, his general state of health being extremely good. Latterly I observed deceased somewhat dejected, very much dejected,—and he frequently complained to me of his embarrassments, making use of the following expression,—"I am suffering under the pressure of pecuniary circumstances." He always appeared painfully affected when he referred to what he had done for supposed friends; and the ingratitude they showed him in his difficulties seemed to be to him equally as unsupportable as the difficulties themselves. The last time I saw deceased alive was on Saturday last (the 24th February), about a quarter to one o'clock in the afternoon; he was then on his own premises, situate No. 18, North Bruton-mews, when he appeared in the same depressed state of mind I had observed him latterly in. I think, however, he appeared a little less agitated; rather calmer. I then went home to my dinner, and I returned about half-past two o'clock to deceased's residence, in North-row, but I found he was still at the premises in North Bruton-mews. At this time deceased's son, Mr. John Boutall, jun., delivered me a message from his father to the effect, that if I had any letters I should bring them to him (deceased) immediately to North Bruton-mews. I instantly proceeded thither, and went up stairs into the front room, on a side-board in which I saw deceased's hat lying. I called deceased by name two or three times; and on receiving no answer I went to the water-closet to see if he were there, but he was not. I then went into a small bed-room, adjoining the front or sitting-room, and there I found deceased reclining on the bed, in a state, I first thought, of temporary repose. He had all his clothes on, except his neck-kerchief, which was unloosened, and the ends of it were lying on deceased's chest. Thinking deceased was only asleep, I called him three or four times; and he making no answer, I shook him by the arm, and called again, but received no answer. I then took deceased by the hand, which I found cold; I afterwards tried to feel his pulse, but pulsation had ceased. I then gave an alarm, and called out to our foreman, William Thomas, who was at work in a room underneath. He immediately came up, and I ordered him to send for a surgeon with all possible dispatch. He sent a person of the name of West, who almost instantly came back with Mr. John

Barrow, sen., surgeon, residing in Davies-street, Oxford-street. Mr. Barrow having examined deceased, attempted, ineffectually, to bleed him in both arms. Mr. Barrow then pronounced my late master dead; that nothing whatever could be done for him; and the doctor, in a few minutes, departed, saying, "the death of deceased was occasioned by apoplexy."* "My own opinion is," said witness, "that Mr. Boutall died of a broken heart. I have heard rumours that deceased had destroyed himself by taking poison,† but I do not believe them."

A volunteer juryman (he had not been summoned, but he displayed considerable anxiety to make one of the jury) of the name, the reporter thinks, of *Bass*, here rose and exclaimed with great warmth, "We shall not allow ourselves to be prejudiced by hear-say reports; we will judge strictly according to the evidence adduced."

The Coroner.—You are bound to do so, and to take no heed of idle rumours.

William Thomas examined.—This witness, without being checked either by the Coroner or jury, went on for a long time giving hear-say evidence, and detailing what the last witness had done, though he (Thomas) had not seen him do it. At last he said, on Saturday deceased desired me, about two o'clock, to allow no one to come up stairs to him, not even Samuel Gould, as he did not wish to be bothered by anybody. However, I disobeyed his orders, and did allow Gould to go up stairs, for I thought there was something the matter with my master. Shortly afterwards Gould called me, and I found deceased in the state described to you by that witness. Mr. Barrow, sen., and another medical gentleman who was called in afterwards, and who is, I believe, son to the former, declared that deceased died of apoplexy. I think so too. [*This witness was not forthcoming at the second inquest.*]

No other witnesses were called, and the jury never expressed a wish to hear the evidence of the surgeon.

The Coroner said the case was a very clear one, and every circumstance tended to show that deceased had come to his death by an attack of apoplexy.

The Jury seemed to think so too, and immediately returned the following verdict:—"Died by the visitation of God."

Remark by the Reporter—It was openly stated, in the room, that deceased had taken prussic acid in consequence of having several forgeries committed by him detected. Persons, not resident in the neighbourhood, were aware of those facts, and how the jury, neighbours of deceased, could be ignorant of them is extraordinary. The verdict,—indeed, the whole of the proceedings, conducted with rapid, and most unscrutinising carelessness,—astonished the reporter, and drew from a gentleman present, Mr. Haydon, of Paddington, the remark,—"Here is another proof of the absolute necessity of a medical coroner."

SECOND INQUEST.

On Friday, a second inquest was holden on the same body, before Mr. Gell, who was assisted on the right hand by Mr. Higgs, the Deputy-Coroner, and on the left by a gentleman whom we understood to be Mr. Price, a legal adviser of the Coroner. After having sharply reprimanded the beadle for having summoned only sixteen jurymen, whereas he had been ordered to summon twenty-four, the Coroner proceeded to explain, at great length, the circumstances under which it became necessary to hold a second inquest on the same body. It would be superfluous to follow the Coroner in the remarks which he made on this occasion. He observed that he had been well satisfied with the first verdict which had been returned, and was much surprised at learning, on Wednesday last, that some gentlemen, dissatisfied with the verdict, were desirous that another inquiry should be holden. He (Mr. Gell) did not think that a second inquest could be taken, but referred the applicants to the Lord Chief Justice, who is Chief Coroner for all England. Accordingly, a deputation of three gentlemen waited on Lord Denman, on Thursday morning; they were received by his Lordship with great kindness, explained the circumstances of the case, and received an assurance that a fresh investigation should be instituted, although it did not appear that any precedent existed for a similar proceeding. The verdict of "Died by the Visitation of God," which was returned at the former inquest, was a perfectly correct one, and conformable with the evidence which had been offered to

* At the second inquest, in reply to a question by a juror relative to this opinion, *Mr. J. Barrow* said,—When I was called in the man was perfectly dead. However, I passed a ligature round the arm, and used the lancet, but no blood flowed. I then tried the other arm—unsuccessfully. Deceased was a man predisposed to apoplexy; he was of large breadth and of very full habit, and had been for several days complaining of pain in the head. On the day of his death he complained, and desired that no one might be shown up to him, as he did not wish to hear of anything that might increase the pain he suffered in the head. My opinion on Saturday was, that he died of apoplexy.

† It will be seen from the evidence of West, in the second inquest, that this witness was aware that a glass and phial smelling of prussic acid, were found; the former under the bed on which deceased was found dead, and the latter in a scullery adjoining the bed-room; they were shown to Gould on the Sunday, and he gave this evidence next day, Monday.

the jury. No blame, therefore, could attach to them, or to him (the Coroner), although, from want of sufficient evidence, they had been put to the inconvenience of assembling together a second time on the same business. Their present object was to determine whether the late verdict was good, and should be confirmed, or whether, as rumour seemed to indicate, the deceased had died from taking poison. In the latter case it would be for them to decide, according to evidence, whether the poison was taken by the individual himself, or administered by another party; finally, they should inquire into the state of the deceased's mind immediately previous to his death; whether he was insane, or temporarily deranged, at the time. At the conclusion of this discourse a juryman inquired if the whole jury should proceed to view the body of the deceased; the Coroner answered that it was not necessary for those who had seen the body on the former inquest to view it again; they might, however, go if they liked; as for himself, having seen the body before, he should not take a second view of it. On the return of the jury, the Coroner said that he would commence by taking the medical evidence.

Mr. J. Barrow, sen., surgeon, of Davies-street, was then sworn, and handed in a written paper, which contained the substance of his evidence. Mr. Barrow had opened the body of the deceased on Thursday (the day previous), and having removed the stomach, in the presence of Mr. Ansell and other persons, had proceeded to examine and test its contents. On opening the stomach the peculiar odour of prussic acid was extremely powerful, and on testing the fluid contents of the organ with sulphate of iron and potash, a peculiar deposit (we suppose prussiate of iron, but the worthy Coroner was unable to *read* the word, and after several efforts said, "Oh, never mind; some deadly poison, I suppose") was thrown down. The witness was, therefore, of opinion that deceased's death was occasioned by a large dose of prussic acid. On being asked by the Coroner if he had examined the head, witness answered, "No, because that could only tend to render the case equivocal."

The second medical witness was *Mr. Ansell*, of Albion-street, Hyde-park. He thought that the evidence of the presence of prussic acid in the stomach (so far as odour went to prove such presence) was satisfactory. A portion of the contents of the stomach was submitted to the usual tests for prussic acid, and, so far as they could be made complete, the existence of the poison was corroborated by the results; but on repeating one of the experiments, on the following morning, that opinion could not be borne out, and, taking the experiments as a whole, he thought that the evidence of the existence of prussic acid was extremely equivocal. The odour was sufficient to enable him to state that there was no doubt of the vapour of prussic acid having issued from the stomach, but on taking home a portion of the fluid contents of the stomach, and submitting them to analysis, the results were not satisfactory. He could not, therefore, swear that death had been occasioned by prussic acid, the more especially as it is known that the acid in question is generated by the decomposition of animal matter. It was just possible that the odour of prussic acid might be found in the body of a person who had died of a natural disease. One fact of this kind had been recorded, after disease of the abdominal viscera.

Coroner.—Do you mean to say that, after the examination which you have made, any doubt remains upon your mind as to the cause of death?

Witness.—From the examination which was made in my presence, I cannot say that no doubt exists in my mind. There certainly is strong presumptive evidence of the existence of prussic acid in the deceased's stomach, but that evidence would have been much stronger had the examination of the body been carried further, in order that we might be able to state positively whether any of the lesions usually produced by prussic acid, did or did not exist in the cavities of the head or chest. The witness, however, concluded by saying that the odour of prussic acid having been found 126 hours after death, furnished not only presumptive evidence that death was caused by prussic acid, but that the quantity taken was large.

A Juryman here asked Mr. Ansell, if any objection had been made to a further and complete examination of the dead body. Mr. Ansell answered that no objection had been made; but that the post-mortem examination had not been continued, because the gentlemen who were responsible (Mr. Barrow, we presume) had expressed themselves satisfied with merely examining the stomach. He (Mr. A.) did not feel himself justified in disturbing the body, as he was not the person appointed by the Coroner. He had attended the deceased for some months previous to his death; the deceased was a man very likely to have been attacked by apoplexy, and had complained of headach about half an hour before his death.

Mr. Barrow, sen., was now recalled, and described what he had seen and done on being called to the deceased. He had endeavoured to bleed the deceased from both arms, but could not obtain a drop of blood. The incisions, however, had given issue to a good deal of blood during the night. No doubt whatever remained on his mind, as to the presence of prussic acid. He had *distilled* the acid contained in the sto-

much, and had obtained, by the tests, the clearest proof in the world.

The evidence of Mr. Samuel Gould, clerk to the deceased, was exactly similar to that which he gave upon the former inquest.

William West, shoeing-smith in the establishment of the deceased, confirmed the facts detailed by Gould and Thomas, in addition to which the following particulars were elicited:—On first entering the room where deceased lay (about 20 minutes to 3 o'clock) the witness was much struck with a peculiar odour, which he had never before smelled. Since then he has smelled some prussic acid, and then recognised that it was that acid which he must have smelled on the first occasion. In passing by the deceased's bed his foot accidentally struck against something; he stooped down, picked it up, and found that it was a wineglass; there was nothing in the glass, but on putting his fingers to the bottom, he found a little moisture; there was a great deal of dust and dirt around the edges of the glass; he immediately showed it to Mr. Barrow, jun., who said that it was nothing. In a scullery adjoining the room in which deceased lay, on a dresser, he also found a small bottle, without a cork, and quite empty; thinks that he showed it to Mr. Barrow, jun., but is not sure. The bottle had no smell of prussic acid when he found it. On the mantel-piece he found a small cork which fitted the bottle; he put the cork to his tongue. (The Coroner did not inquire whether the cork had any peculiar taste or odour, &c.) Mr. Ansell, the chemist (brother to the surgeon who gave evidence), saw it next, in about five or six hours afterwards; he smelt the bottle and desired witness to take care of it, as he was satisfied concerning the contents. There was no one on the premises with the deceased except William Thomas, from a quarter past ten o'clock till two. Thomas was present when the glass and phial were found, and he thinks that Mr. Barrow was there also.

The other witnesses merely deposed to the state of mind in which the deceased was previous to his death. The lawyer who sat on the left hand of the Coroner seemed determined to elicit some point concerning a forgery, and asked a witness (Dunsford) a question to that effect. Mr. Alexander Bartley entered into full particulars respecting certain forgeries which deceased had confessed to him (Mr. B.) that he had committed within the last three weeks; they amounted in all to about £2000. A person named Weedon was the chief cause of all his embarrassment and of his death. Deceased said that he had paid Weedon £2000 within the last year, as interest alone, upon money advanced. (We understand that this same Mr. Weedon was one of the jurymen on the first inquest.) Mr. Bartley was confident that deceased was in a state of temporary derangement for several days previous to death.

The Jury, after a few minutes deliberation, brought in a verdict,—"That deceased took poison while labouring under temporary mental aberration produced by pecuniary difficulties."

STATISTICAL TALENTS OF THE NATIONAL VACCINE BOARD.

To the Editor of THE LANCET.

SIR:—The Report recently issued by the National Vaccine Board contains the following passage:—"In the next place we have the opportunity of bearing our most ample testimony to the continuance of the efficiency of the original vaccine lymph, introduced by Dr. Jenner, through nearly a million of subjects *successively*."

Calculating that an entire week is required for the formation of the vaccine vesicle, to render it suitable for the successive propagation of the disorder, it will be found that a succession of one million of subjects will occupy 19,230 years and 9 months! I am, Sir, your obedient servant,

J. COCKER.

London, March 4, 1839.

FUNCTIONS OF THE LIVER.

To the Editor of THE LANCET.

SIR:—Perceiving in THE LANCET of the 9th a paper by Mr. Ancell, on "The Function of the Liver," I beg to forward you the following extract from the works of Paulus Ægineta, the Greek physician (translated by Francis Adams, Esq.), which, I think, shows that his views are by no means new.

Section LXVII. "According to the views of the ancient physiologists, the liver is the seat of the natural powers, being the grand organ of sanguification, and the blood being the pabulum which nourishes the whole body. That the liver performs an important part in the fabrication of the blood, seems probable from all the veins of the stomach and upper portion of the intestines passing to the liver, whereby it is to be supposed that a considerable proportion of the nutritive juices will be conveyed to it; and from this viscus being proportionally large in the fœtus when it is much required to form blood, and cannot be supposed necessary for any other purpose." In fact the late experiments of Professors Tiedemann and Gmelin seem to prove that the liver is concerned in carrying off the excrementitious part of the blood, or, to use the language of modern chemistry, in decarbonising it. The

ancients taught that the liver, by its *attractive power*, attracts the chyle from the stomach; that by its *retentive* it retains the same until the *alterative* convert it into blood; and then the *expulsive* separates the superfluities of the blood, viz., the bile, and conveys them to the gall-bladder.—See Galenus, tom. ii. page 285. Ed. Basil and Avicenna, lib. iii. fen. 4, tr. i. I am, Sir, your obedient Servant,

J. G. P.

St. George's Hospital,
Feb. 14, 1839.

THE LANCET.

London, Saturday, March 9, 1839.

THE demand which has been unexpectedly made on our space by the reports of the two inquests which have just been held on the body of Mr. BOUTALL, precludes us from publishing the general remarks which we intended to make on the office of Coroner in THE LANCET of this week. The account, however, which we elsewhere present to our readers, of the inquests in question, speaks more strongly to the conviction than any general arguments which we could employ to prove the necessity of electing to the office of Coroner gentlemen who have received a medical education.

WE earnestly direct the attention of our readers to the result of the interview of the British Medical Association with the Poor-Law Commissioners. The insulting system of "Tender" having again been brought into action by the Boards of Guardians in the Windsor Union, the Cirencester Union, and at Halifax, in direct opposition to the recommendations contained in the Report of the Select Committee of the House of Commons of last Session, the Council, at its last meeting, appointed a deputation to wait upon the Commissioners on the subject, and also to inquire what alterations had been effected, or were contemplated in consequence of the Parliamentary inquiry. We rejoice to find that the Council of the British Medical Association are not, like the Councils of some other Associations, asleep at their posts, but continue alive to the importance of this vital question. We can only this week refer to the account of the proceedings of the Deputation so far as they are reported.

THE OFFICE, DUTIES, AND QUALIFICATIONS OF CORONERS.

THE Coroner is an ancient officer in England. He is called *Coroner* because he had principally to do with pleas of *the crown*, or such wherein the King was more immediately concerned. In this respect the Lord Chief Justice of England is the principal Coroner, and may, if he please, exercise the jurisdiction of a Coroner in any part of the realm. There are also particular Coroners for every county in England, in some counties two, and in others a greater number.

This officer is of equal authority with *the sheriff*, and was ordained, together with him, to keep the peace when the earls gave up the wardship of the counties.

Through the culpable neglect of gentlemen of property this office has, in later times, been suffered to fall into disrepute, and get into low and indigent hands.

Although formerly no Coroners were paid, and by the statute of Westminster 1, they were expressly forbidden to take a reward, yet for many years past they have only desired to be chosen for the sake of the perquisites, being first allowed fees for their attendance by the 3 Henry VII., c. 1, of which Sir Edward Coke complained heavily. Since his time Coroners' fees have been much enlarged.

It is unnecessary for us to inform the public that until fees became attached to the office of Coroner *lawyers* were not wont to crave election to it.

Coroners for *counties* (except where custom to the contrary prevails) are chosen by all the freeholders of the county. Formerly

the county Members of Parliament were chosen by the same electors. Now, however, by the Reform Act, the franchise, as respects the election of county members, has been somewhat altered; other interests than *freehold* confer the right of voting for members for counties.

The Reform Act having extended the electors of members for counties it becomes irreconcileable that the election of Coroners for counties should be by the *freeholders only*. The election of a Coroner for any county renders it matter of duty, at least with the freeholders of that county, to consider whether it be indispensable that the gentleman to be elected should be of the legal profession; if it be indispensable that a Coroner should be *a lawyer*, it would become the duty of the county freeholders to vote against the medical candidate, for they are bound not to elect an inefficient officer; but it by no means follows that in the election of a barrister, or an attorney, *a lawyer* would thus be procured to fill the office. But very dangerous to the public welfare would it be, also, if lawyers *only* were deemed eligible for appointment to all offices connected with the administration of *justice*.

In an anonymous advertisement in the "Times," it is *modestly* avowed that in the event of a non-legal Coroner being returned, the greater part of the inquisitions would be quashed for informality, and thus the ends of justice frustrated.

If it be true that a Coroner must necessarily be of the legal profession, that the inquisitions taken before him may be reduced into writing with formal accuracy, it follows that all *sheriffs* and *justices of the peace* should also be of the same profession, these functionaries having numberless forms and technicalities to observe in the execution of their duties.

The education of a gentleman imparts to him sufficient knowledge of law to enable him to discharge the duties of sheriff and justice of the peace with advantage to his country and honour to himself; if he apply himself with diligence to the duties of those offices he will give greater satisfaction to the public when *not* of the legal profession. This is a fact attested by experience. The same truth applies with tenfold force to the office of Coroner.

Lawyers are only adapted to preside over courts where matters of fact and law are gravely argued by gentlemen of the long robe, acting upon instructions prepared with labour and care by solicitors; over courts of *last resort*, where the issue to be adjudicated upon is presented for judgment in orderly arrangement.

The very nature of a lawyer's education leads him to quarrel with a case which is not presented to him in rightful order—in precedented form; to quarrel with witnesses who do not give their evidence with Westminster-hall precision; to quarrel with jurymen who put what to them appear to be ignorant questions to witnesses. Such conduct is most conducive to the ends of justice in courts of last resort, taking cognizance of matters which have already been dealt with in other courts, as a gradation to ultimate trial.

Intermediate tribunals of inquiry are of high utility in all cases of suspected felony, and most especially in cases of sudden or mysterious deaths, or deaths in prison.

The peculiar duty of a Coroner, the only one to which *especial* regard is had upon occasion of appointing him to his office, is to inquire, by a jury over whom he presides, concerning the manner of death when any person is slain, or dies suddenly, or in prison, within the circuit of his jurisdiction.

Public decency requires that in all such cases the Coroner should hold his inquiry with all possible speed; tenderness to the feelings of survivors prompts that such inquiry should not be unnecessarily prolonged.

The manner of holding an inquest is peculiar, and perhaps unavoidably so; the constable of the district, so to speak, scrambles the witnesses together, and every one who imagines he has a tale to tell, either

hastens to the Coroner's Court to offer it in evidence, or he keeps out of the way for a purpose of an opposite nature. It not unfrequently happens, that upon inquiries of this description no person appears to take part in the investigation; sometimes no witnesses are in attendance, except such as the constable has *in his wisdom* summoned.

It is manifest that in a court so convened, offering such imperfect assurance that the required witnesses are in attendance to give evidence, the witnesses cannot be held to that strict line of examination observed in superior courts, without injury to public justice. On the contrary, justice seems rather to require that some latitude should be allowed to witnesses in offering testimony before such a tribunal. If this be so, and it is impossible to doubt the fact, who, we ask, can be so unfit for Coroner as a crotchety and, perchance, legally speaking, a half-educated *attorney?* His strictness would be out of place and season, and tend rather to stifle than develop the truth; whereas, a *non legal* gentleman, proceeding upon the inquiry, in an intelligent, matter-of-fact manner, would not let one feature escape him, in however undigested or irregular a form it might be presented to himself and the jury.

If it be true that a gentleman of education can ably fill the office of Coroner, although not of the legal profession, it follows that *a medical gentleman is the most proper man for the office;* for can it be denied that upon view of the body he would approach his subject with an intelligence which would be of unspeakable advantage to public justice, almost forbidding the possibility of concealment in any case where death has resulted by other than natural causes?—*Morning Journal.*

THE EXCITO-MOTORY SYSTEM.

We have been requested to correct a misconception in the note of Mr. Carpenter in our last Number. Mr. Carpenter imagines that Dr. Marshall Hall had written to M. Flourens, upon the question between them. In this we are assured that Mr. Carpenter is mistaken.

TO THE FREEHOLDERS OF THE COUNTY OF MIDDLESEX.

Gentlemen:—At the close of the contest for the office of Coroner, my grateful acknowledgments were offered on the hustings for the distinguished honour which your support had conferred upon me. I now earnestly repeat the expression of my thanks.

The success which I obtained in the late competition is in the highest degree satisfactory to my feelings. I infer from it that, however zealously I have acted in the field of general politics, the integrity of my motives has not been questioned; and that by your votes and support you have furnished the best evidence of your belief that I shall not abuse the office of Coroner by allowing my conduct to be influenced by any impulses connected with party politics. In entertaining this generous expectation you shall never feel disappointment.

I have now during one week executed the duties of my new situation, and I am more than ever convinced, that in the absence of medical knowledge, the efficient discharge of the functions of the Coroner's office must be the effect of accident. It obviously cannot be the result of a rational and discriminating deliberation.

In conclusion, permit me to assure you that I shall strive unceasingly to prove that your confidence has not been ill bestowed on,

Gentlemen,

Your faithful and obedient servant,

Thomas Wakley.

Bedford-square, March 5, 1835.

BRITISH MEDICAL ASSOCIATION.

The Deputation of the Council alluded to in our last Number waited upon the Poor-Law Commissioners, by appointment, on Wednesday last. A long conversation took place on the subjects which more immediately led to the interview, and also on the operation of the Poor-Law Medical Department generally. The Commissioners expressed themselves as most anxious to do all in their power to remove the grievances of which the profession had complained, and to render the medical department as efficient as possible. With this view they had caused a document to be forwarded to their Assistant-Commissioners for the purpose of obtaining information on various points connected with the working of the present system. This document the Commissioners read

to the Deputation, and though, hitherto, it had not been made public, they would now submit it to the Association for their consideration, and to report thereon. The Commissioners further stated that they would be happy to receive from the Association any plans for improving the medical attendance on the poor, or any suggestions in addition to those elicited during the parliamentary inquiry of last session, that would be likely to give satisfaction to the profession; and they invited the Deputation to another conference towards the end of the present month, previous to settling the medical arrangements for the ensuing year.

The Deputation, therefore, without waiting to give a full report of their interview to the Council, and that no time may be lost, beg, at once, to call the serious attention of their medical brethren to the different points contained in the following document, requesting them to favour the Association as soon as possible with their remarks upon, and in reply to it, and also with any observations or suggestions that may occur to them on the whole question of the Poor-Law Medical Department, addressed (carriage free) to their Secretary at Exeter Hall:—

To ——, Assistant Poor-Law Commissioner.

Sir:—The approaching termination of the parochial year, and the consequent renewal of the medical contracts throughout the Unions, would, under ordinary circumstances have led the Poor-Law Commissioners to have taken this opportunity of deliberating and deciding upon any modification of the existing system of administering medical relief to the poor, which might appear desirable.

Looking, however, to the probability that the subject of the Poor-Law will again come under the consideration of the Legislature in the course of a short time, the Poor-Law Commissioners deem it to be advisable not to originate any immediate or general change in the medical arrangements at the present moment, but to confine themselves to the task of laying before her Majesty's Government the results of the experience of the various systems which are in action in the different parts of England and Wales, and of drawing attention to the conclusions to which the Commissioners, after an attentive consideration of these results, may ultimately arrive.

With this view the Commissioners request that you will bestow the most careful attention on this subject; and that you will furnish them, previously to the 12th of the ensuing month* with a Report containing the several matters herein mentioned; which, with the statistical information already existing in the office will, as the Commissioners trust, place before them the whole of this important subject in all its bearings.

1. How are the medical officers selected in the several Unions under your superintendence, and has the system of tender been adopted in any cases?

2. What mode is adopted in the several Unions under your superintendence of fixing and apportioning the salaries of the medical officers; *i.e.* by a fixed salary; by a payment per case; by a pauper list; or how otherwise?

3. Has dissatisfaction arisen in any, and in which of the Unions under your superintendence, on any of the following points? 1. As to the size of the districts. 2. As to the remuneration of the medical officer. 3. As to the qualification of the medical officer. 4. As to the mode in which he is chosen. 5. As to the attendance or efficiency of the medical officer. And you will distinguish whether that dissatisfaction has arisen on the part of the poor, the medical officer, the Guardians, or the public generally.

4. Have you observed any facts in any, and which of the Unions under your superintendence, tending to show that a disposition to seek medical relief (or relief generally) has been encouraged or discouraged by the medical arrangements?

5. What is the practice in the Unions under your superintendence respecting the medical relief of the families of men who are usually in employment at the ordinary wages of the district?

6. What is the practice in the Unions under your superintendence respecting the medical relief of persons not being able-bodied men, and who are not otherwise in the receipt of out-door relief?

7. Is it the practice in the Unions under your superintendence for aged or infirm persons, or others who are habitually in the receipt of out-door relief, to apply for an order for medical relief before the medical officer of the district attends them?

8. Do you consider any general or special alteration of the medical arrangements in your district is desirable; and, if so, what alteration would you suggest, keeping in your view more especially, the size of the districts; the establishment of a pauper list; the mode of payment of medical officers, and the mode of appointing them?

9. Can you suggest any improvements in the forms of register kept by the medical officers, or in the orders issued by the Board of Guardians or relieving officers, or in the forms of certificate given by the medical officers or other practitioners?

Signed, by order of the Board,

E. Chadwick, Secretary.

Poor-Law Commission Office,
Somerset-house, February. 21st, 1839.

* At the request of the Deputation the time has been extended to the end of the month.

NEGLECT TO FULFIL THE RECOMMENDATIONS OF THE PARLIAMENTARY POOR-LAW COMMITTEE, RESPECTING MEDICAL RELIEF.

To the Editor of THE LANCET.

SIR:—In common with many of my professional friends, I have observed with equal astonishment and regret, the tardiness manifested by the Poor-Law Commissioners in carrying out the recommendations pertaining to medical relief, embodied in the Report of the Poor-Law Committee of the last session.

A considerable period has since elapsed, and had they felt disposed to enforce the resolutions adopted by the Committee, after a full and efficient inquiry, and therefore been entitled to attention and respect, ample time has been afforded them to mature plans and regulations in conformity with the amendments submitted to the Committee, which would have proved conducive to the welfare of the poor, and satisfactory to the profession. But, in truth, I cannot concede to them any such intention, and I know not whether to ascribe their indifference and neglect to a systematic opposition to the profession and a disregard of the poor, or to a culpable pusillanimity, lest, forsooth, they should incur the resentment of arbitrary and irresponsible members of the respective Boards of Guardians (I say irresponsible, because the Guardians *ex officio* direct and control the views of the elected Guardians), who may not shrink from prostituting the powers delegated to them for the behoof the poor, to party and political purposes.

I have been induced to make these remarks in consequence of recent proceedings connected with the Windsor Board of Guardians; they may be thus briefly detailed:—

The medical officer of the Egham district, having felt it necessary to retire, two candidates offered to fill the declined vacancy, myself and a *non-resident*, and the choice of the Board devolved upon the latter, the Guardians consenting to give him 10*l.* per ann. more than the amount of my tender. The plea of economy, so often employed is not, therefore, available upon the present occasion. I addressed a communication to the Commissioners, protesting against the appointment for the following reasons:—1st, because it was injurious to the poor, and unjust to the profession; and, 2ndly, because it was conferred from motives avowedly distinct from a regard for the interests of the poor. I have received a reply, in which they state that they *have made* inquiries, and upon *ex parte* evidence, they arrive at the conclusion, that no grounds exist to justify their interference with the election.

Hence, it is obvious that no improvement will take place, until it be forced upon their attention by some legislative enactment, and I would urge upon the profession the propriety of combining their efforts to attain this desirable end. The profession *have* strongly expressed their marked disapprobation of the existing system, and I deem it a matter of gratulation, as indicative of a growing feeling of unanimity, that of the numerous unemployed junior practitioners, only *one alien* candidate appeared with the palpable object of establishing a private practice, not by fair and honourable competition, but through the denounced medium of the Poor-Law Bill. I am, Sir, your faithful servant,

G. P. HEYWARD.

Egham, Feb. 13, 1839.

REJECTION OF A FAREWELL LETTER FROM

EX-PROFESSOR ELLIOTSON.

A MEETING of students, called by Mr. HOFFMEISTER, took place at University College, on Monday last, for the purpose of hearing read a farewell letter from Dr. Elliotson to his class. Mr. Hoffmeister stated that he had only received the letter on that morning, and felt a desire to peruse it more leisurely and carefully than he had had time to do, before he read it to his fellow-students. He, therefore, requested that the meeting might be adjourned until the following day. This was accordingly agreed to, and on Tuesday the assembly again met, when they were again doomed to be disappointed. Mr. Hoffmeister then stated that he felt it due to the meeting to give a relation of the facts which had transpired respecting the letter. The ex-Professor had written to him, and had requested him to call the meeting for the purpose specified. To this he at once acceded, and on the Monday morning (the previous day) he had breakfasted with the Doctor, who *then* placed in his hands the document to be read. A hasty perusal had not satisfied him as to the contents. Consisting, as the letter did, of 36 pages, he required time to read it carefully. He had done so, and he now felt it to be his duty to respectfully return the letter to Dr. Elliotson, in order that he might get some other party to make it public. He, Mr. H., had been compelled to resort to this proceeding, in consequence of the reflections which it contained on the character and conduct of several of Dr. Elliotson's late colleagues, and friends of the College. He yielded to no man in respect to Dr. Elliotson, but he had respect also for the Professors in that College, and he could not, consistently with that feeling of respect, allow himself to read the letter. He then retired amid much cheering from the meeting.—[It is said that the author of the letter intends to print it.]